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THE
DUBLIN JOURNAL
OF
MEDICAL SCIENCE.

THE BOSTON
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THE DOCTORS'
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THE DUBLIN JOURNAL

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MEDICAL SCIENCE.

JANUARY 1, 1879.

PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*Case of Pachydermatocele.* By ARTHUR WYNNE FOOT, M.D.; Senior Physician to the Meath Hospital and County Dublin Infirmary.

THIS very remarkable case was an example of the pachydermatocele of Valentine Mott—or, as this term is, perhaps, confined rather to cutaneous overgrowths affecting the upper parts of the body, it might be more appropriately named simply elephantiasis, or Barbadoes leg. By a glance at the drawings anyone can see that its right to a title founded on likeness to an elephant's foot is beyond dispute. The skin, prodigiously thickened, hangs in wide baggy folds about the leg and ankle, so that the toes can barely be detected peeping out from under them.

The subject of the case was a young married woman, aged thirty, who was sent up to my wards in the Meath Hospital, from the county Cork, 25th June, 1877. Notwithstanding the size of her left leg and thigh, she was able to stand, walk, and attend to her family and household duties. She could easily move the joints of her toes, ankle, knee, and hip, had full sensibility, and no pain in the limb, and was only inconvenienced by the weight of the massive growths which hung from the femoral region, and by the difficulty of sitting down, in consequence of the protuberance of the principal of these masses. She led a very active life in the country, having five children to look after; she rose at 7 a.m., and

was on her feet till 9 p.m. The principal reason for which the doctor who had seen her at home advised her to come to Dublin was the sake of the rest in bed which could be enforced in hospital, and which, when carried out only for a few days, he noticed to be beneficial. This woman had never been out of the county Cork until her visit to Dublin. Neither of her parents, nor any of their relations, ever had anything similar. She herself always enjoyed good health, and had a strong constitution. She possessed a good appetite, and slept well, and had had a child four months before she came to Dublin, which she nursed up to three days before her admission.

The disease had existed for nine years when she came under my observation. Her history of its origin was that a week after having got a severe wetting she felt a sharp pain in the groin, which ran down the front of the limb to her toes. The pain was attended with a swelling of the limb, which continued after the pain (which lasted for a few days) had gone away.

Since that time the limb had never regained its proper size. At the time the pain was felt she did not observe any enlarged glands in the groin, but has often done so since then, and always in the left groin. She was married twelve months after the commencement of the disease, at the age of twenty-one, and had had seven children, including twins once. Five of her seven children are living. The twins, which were seven-months' children, both died soon after birth. She had no difficulty in any of her confinements, and nursed all her children but one. During each pregnancy the limb used to get larger, and become reduced in size immediately after delivery. It swelled much more than usual when she was carrying the twins. From the commencement of the disease up to the date of her marriage the whole limb was continually swelled, and used to pit on pressure, but, as it did not interfere with her movements, she did not mind it at all. During the last two or three years, she said, "things like small boils" appeared more than once, generally just before confinements, when her leg was most swollen. These "boils" used to first discharge matter, and afterwards water. They generally dried up in less than a week. The discharge from these "boils" used to reduce the size of the limb, but at the same time weakened her. On one occasion she got a scratch from a brier on the lower part of the leg, which gave exit to a quantity of water, and temporarily lessened the size of the limb. On three or four occasions she had had erysipelas; the last bad attack was three

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years before admission, and lasted for three or four weeks. At such times she could not move or lift the leg.

The following measurements were made 26th June, 1877:—

	LEFT.	RIGHT.
	Inches.	Inches.
Base of toes - -	8½	8½
Middle of foot -	11	8½
Above ankle joint -	19¾	8½
Lower third of leg -	22¼	9½
Calf of leg - -	26¾	12¼
Below knee - -	25¼	11¾
At knee - -	30½	13¼
Middle-third of thigh	49½	14½
Upper third of thigh	23½	17¼

Notwithstanding the enormous bulk of the left limb, she carried it off very well, and when she was up and dressed the deformity made surprisingly little show. She walked about, and up and down stairs, without a stick or assistance of any kind. In standing, “the small leg” (right) fitted into a groove in the large one. When she lay on her right side (her usual position) the right thigh was completely hidden by the cutaneous overgrowth connected with the inner surface of the left thigh, while the masses springing from the outer aspect of the left thigh raised the bedclothes in a high mound, and those connected with the posterior femoral region lay stretched out on the bed far behind her. The gluteal region was quite unaffected, as also were the organs of generation. Her principal difficulty was in sitting down, owing to the inconvenient protuberance of the mass attached to the posterior femoral region. Her most comfortable position was on a low stool, in which attitude the large posterior mass rested on the ground, from contact with which the skin there was rough and scabrous. Defecation and micturition, as she could not sit down on any utensil, were managed by holding out of the bed-post and discharging into a vessel placed behind her. In her long railway journey to Dublin she lay along the seat of the carriage the whole way.

The skin of the diseased leg was in general smooth, except where it came in constant contact with her clothes or the ground in sitting, and here it was scabrous and warty, and covered with thorny projections, which made it look like an ox’s tongue; the holes of the sweat follicles were very wide and stretched far apart, and the

intervening areas glazed. There were many deep sulci, or creases, where the lines of flexion existed; in such parts the skin was soft and moist, and a very heavy rancid smell was exhaled from these fissures, though she kept these places washed as well as she could.

As neither the patient nor her husband would hear of any operative interference with the limb, she was retained in hospital more as a curiosity, and for the sake of observation, than for any other object; but, meanwhile, rest in bed, elevation of the limb, and compression with elastic bandages, were constantly employed, while diuretic medicines were exhibited. The effect of this treatment was soon apparent in a great relaxation of the principal mass which was attached to the posterior femoral region, which, from having been almost as firm as a camel's hump, became a flabby pendulous appendage—the dermatolysis of English writers. As she was assured that this would fill up and get firm again when she got up and discontinued the treatment, she consented, after much hesitation, to have it removed; and we were in hopes, if the operation were successful, and that a firmly-contracted cicatrix were substituted for loose folds of relaxed integument, that at another time a second large portion of the redundant integument might be removed. During the performance of the operation the venous hæmorrhage was unexpectedly profuse, owing to the division of the almost plexiform series of large veins which ramified in the subcutaneous layer, and this loss of blood, combined with an unusual degree of chloroform-sickness, and much depression from shock, acting on a very nervous temperament, contributed to bring about a fatal issue on the fourth day after the operation.

The portion of integument removed weighed many pounds; it presented evident hypertrophy of the cuticle, the corium, and the subcutaneous cellular tissue. The numerous blood-vessels, chiefly veins, gave the deeper parts of the structure the appearance of almost a cavernous metamorphosis, their lumina resembling the channels excavated in worm-eaten wood.

In reference to the name of this disease, which, though so easily recognised, has been hampered with a multiplicity of titles, there is no doubt of the synonymy of *Pachydermia*,^a *Pachydermatocèle*,^b *Dermatolysis*, *Elephantiasis* (Arabum), *Spargosis*,^c *Barbadoes leg.* There is a good drawing of the pendulous form of the

^a Wagner. *Man. Gen. Path.* 1876, p. 383.

^b *Med. Chi. Trans.* Vol. XXXVII., 1854.

^c E. Wilson. *Lects. on Dermatology.* 1874-5, p. 173.

disease, called Pachydermatocele by Professor Valentine Mott, in a paper by Mr. Stokes,^a and another in a paper on two cases of Dermatology, by Dr. G. Fritsche.^b

It is noteworthy that the patient here referred to attributed the origin of the disease to a severe wetting. The Chinese believe this disease, which is common at Ningpo, to be occasioned by wading in the mountain streams chilled by ice. In 340 cases of elephant leg, Mr. Waring found the average size round the ankle to be $11\frac{7}{8}$ inches; in this patient it was $19\frac{3}{4}$ inches. The disease is more common in males than females. According to Mr. Waring's observations, 75 per cent. of cases were males and 25 females.

ART. II.—*On the Consolidation of Internal Aneurisms.* By JOLLIFFE TUFNELL, M.R.I.A.; Consulting Surgeon to the City of Dublin Hospital; &c.

IN a comparatively recent number of this Journal^c the following opinion has been advanced in reference to the cure of aneurisms—viz., “That the layers of lymph deposited in the sac of an aneurism are formed by the walls of the sac itself, and are not a deposit from the blood.”

Sorry as I am to be obliged to differ from so distinguished a surgeon as the writer of this article, still I cannot admit the correctness of any one of the arguments which he has advanced in support of his views; and I must maintain and hold to the fact (as demonstrated in every pathological museum) that the consolidation and cure of an aneurism is not the result of any secretion from the sac itself, but a purely mechanical deposit—rapid when the sac is occluded by clots, and slow and progressive when consolidated by fibrin deposited in successive layers.

I affirm that in neither case is inflammation present, and that in no instance does lymph (the product of inflammation) act as the agent, or constitute the medium through which cure and recovery are brought about.

In the case of an aneurism treated by ligature, where the circulation is suddenly and almost entirely cut off, and rapid consolidation

^a Dub. Journal of Med. Science. Vol. LXI., p. 1.

^b Clin. Soc. Trans. Vol. VI., p. 163.

^c Vol. LXIV., No. 70, October 1, 1877. On Aneurismal Sacs. By William Colles, Surgeon to the Queen in Ireland; Regius Professor of Surgery in the University of Dublin; Surgeon to Dr. Steevens' Hospital, &c., &c.

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ART. III.—*Notes on the Treatment of Chorea.*^a By THOMAS HAYDEN, F.K.Q.C.P.; Physician to the Mater Misericordiarum Hospital.

OUR knowledge of chorea, in regard to its pathogeny and its pathology, is eminently vague and indefinite—hence the tentative and often contradictory plans pursued in its treatment; but that, under the increasing light now being shed by the inductive method of inquiry in the domain of nerve-physiology and pathology, the hidden causes of chorea will soon be revealed, I cannot doubt. Till that advanced point shall have been reached, it is our duty to proceed by such light as we have, or by the empirical method. As Von Ziemssen says, we must test our remedies “as objectively as possible” in each case, with special reference to the individuality of the patient.

But pure empiricism is scarcely known in modern medicine. In the treatment of any given disease we all have before our minds some theory, more or less definite and connected, in reference to it, upon which we proceed. It is the duty of every practitioner to see that his theory is, as far as may be, in consonance with the actual knowledge of the disease already gained.

I cannot think that any of the modern theories, implying a special organic lesion as the exciting cause of chorea, will bear scrutiny. Certainly the embolic and thrombotic theories, propounded by Kirkes and Hughlings Jackson, will not pass unscathed through the light of an extended hospital experience.

CASE I.—Mary Anne Corley, aged eleven, admitted November 29, 1877; had been in the hospital two years previously for a severe burn of the face, resulting from the ignition of paper used for curling her hair; one whole side of her face had been badly burned, and was cicatrised at the date of her second admittance. Four days previous to that date she was frightened by seeing her father come home drunk at night, and immediately afterwards became choreic, but for some time previously she had been in a state of continued alarm from this cause. When visited by me on the 30th November she was suffering from general chorea of an aggravated character, and had not slept for ninety-six hours previously. She was thin and fatuous-looking, and lay on her back, apparently powerless; the limbs were almost constantly agitated—the arms suddenly

^a Read before the Medical Society of the King and Queen's College of Physicians in Ireland, on Wednesday, December 4, 1878. [For the discussion on this paper see p. 41.]

flung about in a disorderly manner and with great violence, the legs alternately flexed and extended, the face exhibiting the most unsightly grimaces, and the tongue, at my request, protruded for a moment only after a prolonged effort, and then suddenly retracted. Deglutition, even of liquids, was accomplished with great difficulty, and articulation was scarcely intelligible. The back of the shoulders and elbows, the sacrum and the heels, were already frayed by friction against the bedding. The action of the heart was quick and feeble, but regular; there was no cardiac murmur.

Ether-spray was used along the spine, and subsequently the ice-bag was applied with no perceptible benefit; one-sixth of a grain of morphia was injected hypodermically.

Dec. 1st.—Had had no sleep; two draughts were prescribed, consisting each of 10 grs. of chloral hydrate, 10 m. of solution of the hydrochlorate of morphia, and 10 m. of compound tincture of chloroform—one to be given at 6 p.m., and the second, if necessary, at 10 the following morning.

2nd.—Had taken both draughts and slept a few hours.

3rd.—To have 3 m. of Fowler's arsenical solution, with ʒi of syrup of the iodide of iron, every fourth hour.

5th.—No improvement; to have gr. 15 of bromide of potassium, with 20 m. of aromatic spirit of ammonia, every fourth hour.

15th.—Improved; pulse 48, and very weak; last medicine to be stopped, and ammoniated tincture of valerian, and chloric ether, of each 10 m., to be given every third hour.

19th.—No improvement; to have 10 m. of the tincture of Indian hemp, with gr. 15 of chloral hydrate, every third hour.

28th.—The patient was now reduced to an extreme state of emaciation, but slightly improved in regard to the choreic movements; to have, instead of previous medicine, 5 m. of ethereal tincture of phosphorus, with 3 m. of the solution of strychnia (B.P.) every third hour.

29th.—Exhibited a decided improvement, which from this date continued progressively and without a check, to complete recovery. The limbs were wasted to an extreme degree, and the child, though soon able to leave her bed, was unable to stand from weakness.

Jan. 11th, 1878.—Phosphorus and strychnia suspended, and ʒii of cod-liver oil, with ʒi of syrup of the iodide of iron now given thrice daily.

23rd.—Patient now fat, quite free from chorea, and walking about the ward. Discharged February 3rd.

Nov. 25th, 1878.—She came to the hospital to-day to "show herself;" was robust, healthy-looking, and without a trace of her former disease.

The total duration of the child's illness was forty-seven days; and the other medicines mentioned having been fairly tried with little

or no benefit, complete recovery was effected in fourteen days under the use of phosphorus and strychnia.

CASE II.—Anne Conlon, aged eleven, admitted under my care for chorea, May 13, 1878. Her father had died of phthisis five years previously, at the age of sixty; mother still living and healthy; she was herself the youngest of thirteen children, six of whom were then alive, and all healthy except the eldest, who was reported dyspeptic. The patient had scarlatina at the age of two years, measles at nine, and whooping cough at ten. Two years previously she had an attack of chorea which lasted a week; it was caused by the shouting of a drunken man, and began with pain below the right knee, followed by numbness and spasms in the right foot, and afterwards in the right hand. Two days later the left arm and leg became affected.

She had a second attack in the summer of 1877; for this she could assign no cause, but it was more severe than the first, and lasted a month, the mother noticing an indistinctness in her speech.

The third attack, for which she was admitted, began a month previously, and was, according to the mother's report, more severe than either of the preceding. On the day of her admission she was unable to speak for some time, and could swallow even liquids with difficulty. Apex-pulsation was felt half an inch inside the nipple-line, and here a sharp and loud bellows-murmur was audible after the first sound.

To have arsenical solution 3 m., with syrup of the phosphate of iron 3i, three times daily.

May 18th.—No perceptible improvement; previous medicine stopped, and ethereal tincture of phosphorus with liquor of strychnia, of each 3 m., given thrice daily; shower-baths.

July 13th.—Nearly well; has gained flesh and colour, and of the chorea slight fidgeting of the hands and arms only remains. Discharged.

Duration of illness, ninety-one days; of treatment by phosphorus and strychnia, fifty-six days; result, recovery nearly complete.

CASE III.—Marian Heuster, aged nine, was admitted for chorea, June 29, 1878. Had been healthy up to two months previously, when, after having a tooth drawn, she was suddenly attacked with chorea.

When admitted she was in a fair state of flesh; there was extreme agitation of the arms, also contortion of the features; deglutition unaffected; pupils dilated; pulse 108; heart's action regular and unattended with murmur.

To have 8 m. of the tincture of phosphorus and of the liquor of strychnia thrice daily; also a tepid shower.

July 6th.—Medicine and shower baths continued; made slow but

uninterrupted progress towards recovery, and was discharged perfectly cured on the 16th of July.

Duration of illness, seventy-seven days; of treatment by means of strychnia and phosphorus, seventeen days; result, cure.

The attack is, in most instances, directly traceable to fright or other emotional excitement of a depressing character, operating upon a nervous and feeble constitution, and at a period of life when the receptive faculties are most sensitive. If I must have a theory of chorea, I should say that the attack commences with vasomotor paresis, resulting from a profound emotional impression, and that the essential symptoms are due to defective polarity or dynamic instability of the motor nerve tracts, both intracranial and spinal. On this hypothesis the vascular congestion, central and peripheral, of the brain and cord, with occasional extravasation, and in inveterate cases, central sclerosis, noted by Dickenson, might be explained.

The history of most cases of chorea, and the success attending a nutritive and tonic plan of treatment, might also be adduced in support of this view of its pathology.

Reasoning from the foregoing premises, I concluded that phosphorus and strychnia combined—the former a nerve-nutrient of recognised value, and the latter a nerve-tonic of great potency—might prove efficacious in the treatment of chorea. As yet my experience of this plan of treatment has been very limited, extending only to three cases, but so far it has been eminently satisfactory, and I venture strongly to recommend it for further trial. The first of the cases above reported would inevitably have been lost under any other treatment known to me, and death was close at hand when phosphorus and strychnia were given, in the last resort; under the use and through the efficacy of these agents the child was, within the short period of fourteen days, cured of the disease which had previously defied treatment.

Both strychnia and phosphorus are already familiar to physicians in the treatment of chorea. Trousseau had great confidence in the former, pushed to its toxic limits; and the latter was given many years ago by Radcliffe, and is, I believe—or its equivalent, hypophosphite of soda—still given, and strongly recommended, by that eminent physician; but I am not aware that strychnia and phosphorus combined have been previously administered for the cure of chorea. This is, however, a matter of minor importance in view of the therapeutic result.

ART. IV.—*Warburg's Tincture, and its Therapeutic Value in Indian Fevers.*^a By WILLIAM OWEN, M.B. (Univ. Dubl.), L.R.C.S.I., L.M. (Rotunda Hospital), Bengal Medical Staff; Civil Surgeon, Dehree-on-the-Soane, Behar, Bengal.

It seems surprising that a medicine which has been in existence for a length of time should only now be attracting the attention it deserves, and it is no doubt curious that a scientific investigation as regards its composition, and more especially the *rationale* of its action, should have been made only recently.

A secret remedy, the discovery of a German Jew residing in British Guiana, its composition was at last divulged, and it was found to be of a most complex nature, containing, among others, the following ingredients:—Aloes, rhubarb, angelica seeds, confection of Damocratis (various aromatics), radix helenii, saffron, fennel, gentian, zedoaria, myrrh, cubeba, chalk, camphor, boletus laricis, and quinine in large amount. It appears to have been used to a considerable extent in the treatment of malarial fever in Southern India, but a scientific trial of it in the treatment of fever was reserved for modern times, and among the best known and most notable experimenters we find the names of Drs. Moorhead and Maclean, both celebrated in the annals of Indian medicine. Of these, the former speaks slightly of the remedy; not so, however, the latter, for in his article in "Reynolds' Medicine" on Remittent, he says, referring to this drug, "I have given this tincture a fair trial in some of the gravest forms of malarial fever, and it has been extensively used by some of the most experienced officers of the Madras army, and I do not hesitate to say that I think it a valuable remedy." He adds a few more lines describing its effects.

This scanty notice would not convey to the reader's mind an idea of the value which the author sets on this medicine as a therapeutic agent. To estimate this aright it is necessary to attend a course of lectures on military medicine at Netley, and hear Dr. Maclean devote one lecture to this subject. There he insists with great earnestness on the utility of the drug, and points out forcibly what a powerful instrument we have in it to combat the too often fatal malarial fevers of tropical countries.

The notice of this remedy by authors of books on medicine and therapeutics of the present day is of the most meagre description,

^a Being a Thesis for the degree of Doctor of Medicine of the University of Dublin.

and quite inadequate to the importance of the subject. Thus, in Dr. Aitken's "Practice of Medicine," the only mention of Warburg's tincture to be found is in a quotation from Dr. Maclean's article on Fever in "Reynolds' System," while in Waring's book on "Therapeutics" a few lines only are devoted to this subject. Dr. Roberts (*vide* "Practice of Medicine") enumerates it among others as "a remedy which has gained much repute in the treatment of remittent fevers"—other works on medicine and therapeutics quietly ignoring the existence of this curative agent.

When the standard works take so little notice of this remedial agent, it is not to be wondered at that men who know only of it from the account given by these books should use it hesitatingly, or in many cases not use it at all; and consequently it is not surprising to meet practitioners, as I have done, who will frankly tell you they have never heard of the drug—while others, when perhaps you propose its administration in a bad case, will say, "Oh! I have heard of that medicine, but don't know much about it. I believe it is a patent medicine, and most uncertain in its action, and is not safe to administer to children." Having myself fortunately had the advantage of attending Dr. Maclean's lectures on this subject, a deep impression was made on my mind; and being convinced of the great power of this drug from the account of it given by Dr. Maclean—a man of thirty years' Indian experience—I was determined to give it a fair trial on every opportunity.

My first appointment in this country (India) being the medical charge of a railway over 200 miles in length, through a country opened up for the first time, and of a low-lying and swampy nature, the number of fever cases was quite unlimited. The northern extremity of the railway which was under construction entailing excavation of the ground, and being situate in a very jungly district, produced cases of a true and most severe form of remittent fever. The opportunity thus afforded me of estimating the value of Warburg's tincture has not been lost. My experience leads me to set a high value on it, and I believe it to have been the means of saving life in my hands. I can fully bear out everything that I have heard Dr. Maclean say in its favour. I have used it in intermittent as well as remittent fever, and prescribed it to children and adults—in all with the same beneficial results. Out of many it will be sufficient to give a short summary of one case which is typical, and then draw some few conclusions which the results of the whole number of cases treated seem to warrant.

I was called in one day to see a native, aged nine years. When I arrived I found him suffering from a severe attack of remittent fever. It appeared that the native doctor had been treating him for five days, and in consequence of the case getting worse instead of better, I was called in. The chief treatment employed up to this time had been quinine, in doses of grs. 2, three times a day, with diaphoretics.

On my arrival I found the patient with the following symptoms:—A rapid and full pulse, carotids throbbing, heart's action tremulous, temperature over 105° Fahr., skin dry as parchment, tongue dry, great thirst, irritability of the stomach, together with pain and tympanitic distension of the abdomen. In addition to these, the bowels were slightly loose and the spleen considerably enlarged.

Five days afterwards the symptoms were as follows:—Almost constant delirium, with a tendency to coma, eyes deeply congested, pupils contracted, tongue dry and brown, teeth covered with sordes, urine very scanty in amount, concentrated, and very high-coloured; diarrhoea. At this time the temperature was as high as before, with a scarcely perceptible remission, while the pulse, in addition to its rapidity, had become weak and thready. Double bronchitis had also made its appearance.

On the second day of my attendance a native assistant surgeon, who had had much experience in fevers, was called in in consultation. During the first five days of my attendance the following was the treatment adopted:—Quinine was given to the extent of grs. 90; and on one day grs. 40 were administered, with the object, if possible, of breaking the continuance of the fever, and producing a distinct remission, but this it failed to do. Diaphoretics of various kinds were given; the body was frequently sponged with tepid vinegar and water—at the same time baths, in which the temperature of the water was gradually reduced, were tried without effect. Soup, milk, lime-juice and water, together with stimulants, were taken by the patient. The local measures employed were shaving the head and applying cold lotions, poultices, and counter-irritation to the chest, turpentine stupes to the abdomen and stomach, and warm and stimulating pediluvia.

On the night of the fifth day—the state of the child being very serious, and the parents being very anxious about him—the native assistant surgeon was called in for the second time, and a consultation held. He acknowledged the gravity of the case, and failing to suggest any further remedial measures, I bethought myself of

Warburg's tincture, and at once proposed that it should be administered. He said he had once heard of its being used, but never prescribed it or saw it prescribed; but as the condition of the child was becoming worse, and all the remedies hitherto employed had produced no results, he did not object to a trial of this medicine. At this time I was out of the drug, but a gentleman living in the station, on whom I had used it in fever with success, and who was never without a supply of it, kindly lent me a bottle.

Early on the morning of the sixth day one drachm of the tincture was given, with directions to repeat the dose every two hours until free perspiration was produced. In the meantime all drink was to be withheld. Two doses produced the desired effect, causing free but not excessive perspiration, relieving the head symptoms, and bringing down the temperature two degrees—i.e., to 103° Fahr. Quinine was then administered in doses of grs. 3 every two hours. Next morning the temperature was still at 103°, with a slight return of head symptoms, and I considered it necessary to repeat the tincture as on the day previous. Perspiration was the result, with a bringing down of the temperature to 100° Fahr. The unfavourable symptoms also disappeared. On this day grs. 9 of quinine were given. After this the exacerbations were much reduced in intensity and frequency as well as in duration, and the remissions became quite distinct; and by the judicious use of quinine daily the temperature was brought down to normal in ten days, and the child made a most complete recovery.

Whatever the *rationale* of the action of this drug may be—whether by means of the powerful aromatics which it contains, it acts as a stimulant on the nervous system, thus enabling it to throw off the poison through the skin, or other excretory channels; or whether by its diaphoretic action it displaces the poison from the liver, spleen, and other organs, and brings it in contact directly with the quinine (which is present in large amount and must be absorbed in common with the other constituents), thereby producing a direct destruction of the morbid material, matters little—its power for good remains. Whatever the malarial poison may be, or whatever its composition, we know of it, I may say, only by its effects on the human system, and by what we find in the organs of those who have long been subjected to its influence, or who have died from malarial poisoning (fevers, &c., &c.). This poison appears to have a peculiar affinity for the liver, spleen, and glandular apparatus of the intestinal tract, producing pigmentation—this being

greater or less according to the length of time the individual may have been exposed to the noxious influence, and also in proportion to the strength and concentration of the poison. This poison, evidenced by pigmentation, must have some time during which the pigment is deposited, and it is probable that during attacks of intermittent and remittent fevers this process is actively going on, producing, by means of irritation, enlargement of the liver, or spleen, or gastro-intestinal derangement, according to the organ for which the poison in each individual case exhibits the most affinity. Consequently, I have been led to think my latter theory, as to the *modus operandi* of the drug in exerting a curative effect, as not improbable.

In the case related by me the native assistant surgeon agreed with me that without this drug the termination would have been fatal. The same opinion has been formed by me in other cases. It is this which has led me to write about, and record my limited experience of, a drug little known, and still less appreciated, in the hope that at no distant date it may be accorded that high place among remedial agents (for malarial fevers) which is its due.

The following are the conclusions to which I have been led from a careful observation of the cases which have come under my notice:—

1. That Warburg's tincture is a remedy of great value in remittent fever—in some cases preventing a return of the exacerbations; in others, and these the most numerous, diminishing the intensity of the exacerbations, and rendering the remissions distinct, thereby lessening the force of the fever, and opening the way for the subsequent beneficial action of quinine.

2. That in bad cases of intermittent fever it often acts as a charm—in some cases dispersing the paroxysms, not to return; in others diminishing their force and lessening their duration.

3. That it appears to act on the fever *per se*, at the same time increasing the subsequent beneficial action of quinine in a marked degree.

4. That it may be administered with perfect safety by competent hands to children as well as adults.

5. That moderate perspiration produces as good results in these cases as excessive, and the former can, if necessary, be repeated by repeating the dose.

6. That excessive perspiration and consequent debilitating effects

(this urged as an objection against it) may be prevented by regulating the dose.

7. That in remittent fevers of a typhoid type with high temperature this medicine may be administered with great advantage, care being taken that the dose and subsequent perspiration be not excessive, and any symptoms of debility being combated with stimulants.

ART. V.—*The Generation of Typhus from Overcrowding.* By JAMES MARTIN, F.R.C.S.I., &c.; Physician to the Portlaw Fever Hospital, &c.

A FEW years ago I published a memorandum in this Journal,* going to prove that typhus fever may arise as an ochlesis, and that on that occasion its origin might be sought for either in a correlation with measles, or from overcrowding and the accumulation of bodily emanations.

I now have a case to report in which the origin can only be attributed to the latter. We had no fever in the district for over twelve months. Typhus was rare during the last year in the hospital of Waterford, ten miles from us; only one case had been admitted to the Carrickmines Hospital for several months, six miles from us. No communication could be traced in any way. The family in which the outbreak I now allude to occurred, consisted of father, mother, and seven children, one only three weeks old. Since the mother's confinement, the six elder children—one a girl of fifteen, all the others under thirteen—huddled together in one bed of straw on the floor of a loft, in consequence of want of bedding and the coldness of the weather. The eldest was seized with fever and removed to hospital on the fifth day, and proved a heavy case of maculated typhus. The beds were burned, additional bedclothes procured, but there was no disinfection of the clothing. The mother took ill and was admitted eight days after the admission of the daughter; four of the younger children were admitted also. The baby and the child above it in age escaped.

The mother was a bad case with heavy typhus rash; retained her consciousness too clearly; had no marked lesion; the thermometer never ran very high, and yet she sank on the twelfth day, apparently from general failure of nervous power. None of the young children showed any eruption, and they have all recovered. The father was the last admitted, proving a heavy case of maculated

typhus, with sharp head symptoms, but he has also made a good recovery.

My inferences from this and former cases are—

1. That typhus may arise spontaneously from personal dirt and overcrowding.

2. That it is peculiarly dangerous to women soon after confinement.

3. That it is seldom very severe on children—that they seldom, if ever, have the typhus eruption, and that they generally recover.

4. That the period of incubation is about ten days.

5. That when one member of a family has become affected, the only chance of preserving the remainder is to institute perfect ablutions of all the remaining members; to burn beds, to provide clean bedclothes, and thoroughly disinfect and wash, if possible, all the old bedclothes and garments.

RECENT VIEWS ON DEVELOPMENT.

MR. F. M. BALFOUR, M.A., in the *Quarterly Journal of Microscopical Science*, gives an account of the most recent views on the phenomena accompanying the maturation and impregnation of the ovum. The following summary shows the series of events in the order of occurrence:—1. Transportation of germinal vesicle to surface of egg. 2. Absorption of membrane of germinal vesicle and metamorphosis of germinal spot. 3. Assumption of a spindle character (or form) by remains of germinal vesicle, and largely from germinal spot. 4. Entrance of one end of spindle into a protoplasmic prominence at surface of egg. 5. Division of the spindle into two halves, one remaining in the egg, the other in the prominence. The prominence at the same time nearly constricted off from the egg as a polar cell. 6. Formation of a second polar cell in the same manner as first part of the spindle still remaining in the egg. 7. Conversion of the part of the spindle remaining in the egg into a nucleus—the female pronucleus. 8. Transportation of the female pronucleus toward the centre of the egg. 9. Entrance of one spermatozoon into the egg. 10. Conversion of the head of the spermatozoon into a nucleus—the male pronucleus. 11. Appearance of radial striæ round the male pronucleus, which gradually travels toward female pronucleus. 12. Fusion of male and female pronuclei to form the first segmentation nucleus. The contents of the germinal vesicle at maturity and before impregnation are, for the most part, fluid, but may be granular. Their most characteristic component is, however, a protoplasmic network, which stretches from the germinal spot to the investing membrane.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Dental Materia Medica and Therapeutics, with Pharmacopœia.
By JAMES STOCKEN, L.D.S.R.C.S., Eng. Second Edition.
London: J. & A. Churchill. 1878. 8vo. Pp. 319.

WE learn from the preface to the second edition of the above handbook that its scope has been enlarged in order to meet the want of the dental student preparing for an examination in *materia medica*. With this end in view, the author has attempted to give a *précis* of general *materia medica*—representing, we may infer, the amount of knowledge likely to be required of the dental student in that branch of study. Mr. Stocken has thought fit to devote many pages to the discussion of the treatment of poisoning—a startling, though perhaps a wise precaution. So much, indeed, is the book taken up with generalities, that the special information, of which the title of the work gave promise, is to the last degree meagre and inaccurate. To one who, with a fair knowledge of general *materia medica*, approaches the book in the hope that he may learn something special, there will come, on its perusal, a thorough disappointment. Nor do we think that he who approaches the book from the dental side—that is, from the position of one who, ignorant of general, may be versed in special *materia medica*—will fare much better. For, in common with all small books, it will be found very much harder to master the contents of this one, cut and dry as they are, than those of larger works, which handle their subject in a fuller and more entertaining manner.

But if we have found the title of dental *materia medica* misleading, we shall find in the search after the promise of the therapeutical factor of the title still greater disappointment. Any stray crumbs of special information are so hopelessly lost in the tangled mass, that without the aid of an index, of which there is none from the side of therapeutics, there is no chance of the reader benefiting by them.

Without entering into any of the minutiae of Mr. Stocken's errors of omission, the shortcomings of the work may be instanced by a reference to three or four glaring defects.

Turning to the therapeutics of "white arsenic," we find the following sentence:—"It is employed to devitalise the dental pulp, and also to obtund the pain of sensitive dentine." And then follows a description of how the arsenic is to be used "for this purpose"—*i. e.*, for obtunding sensitive dentine. The reader who wishes to find out how to use arsenic for the one thing for which it is almost a specific—namely, the devitalisation of the dental pulp, will, after a perusal of Mr. Stocken's remarks, be left in entire darkness.

Again, take the important drug, "mercury." We might expect to find some allusion under this heading to "mercurial teeth," but no reference to this condition finds a place in Mr. Stocken's pages.

The question of bleaching teeth, and the agents to be employed with this end in view, ought to find some place, if not a very prominent one, in such a work as the present professes to be, but on this point the work is silent.

A prominent place should certainly be given to the discussion of anæsthetics, and Mr. Stocken has made some attempt towards this end. His remarks on ether and chloroform are, however, very brief and meagre; and though he enters into more detail, adduced chiefly from Dr. White's book, when discussing laughing gas, he is guilty here of what we believe to be an important omission—*viz.*, he never alludes to the danger of administering the gas a second time after a short interval.

Mr. Stocken's work is, to a large extent, a compilation from other authors; but in some of his applications of the axioms of general therapeutics to his own speciality, he is not very fortunate. Thus, when alluding to food and exercise, he fails to point out how, in these two matters, the organs with which dentistry is chiefly concerned occupy an isolated position. The relation of the ill-developed jaw to overcrowding of the teeth and fatal approximal decay is now pretty generally admitted, yet Mr. Stocken fails to inculcate how important an agent, in the formation of the jaws, the exercise of the muscles attached to them may become, and he fails still more culpably to allude to the direct action of food on the teeth.

It is painful, indeed, to have to find unmitigated fault with any work, and perhaps the last twenty pages of that before us may be

considered to possess something of a redeeming quality by those who wish to be saved the trouble of original prescribing.

Altogether, while we think there is ample room for a work which would bear out in its pages the pledge contained in the title of Mr. Stocken's book, we think that the latter entirely fails to do so, and we can only recommend its perusal, as a matter of exigency, to a student cramming for the London examination.

Sore Throat. By PROSSER JAMES, M.D. Third Edition. Pp. 288. London: J. & A. Churchill. 1878.

"SEVEN years since, in compliance with the wishes of pupils and friends, I expressed a determination to revise the book." The result we have before us. If it is to prove, as the author hopes, "useful to my professional brethren," we would recommend him to revise the book again, if not completely to rewrite it. To avoid circumlocution, the author tells us he has not avoided the use of the pronoun "I." In this assertion we are inclined to agree with him, though we cannot extenuate the use of the pronoun, so often and so prominently brought forward, on the grounds which Mr. James pleads. Professorial labour has not always this tendency. Most of our best works have been written by men to whom professorial work has been no stranger, and in them we find the love of their profession has caused the person to be placed in the background, whilst the results of their labours and those of others occupy the chief place. It is not so in this book. I, "the first English worker with the laryngoscope"—"I had *myself* succeeded in detecting and curing disease by the aid of reflected light, and thus in reality applied the principle of the laryngoscope before the date of that invention" (page 52). In revising the earlier editions of the book before us, Mr. James says, "It has been a gratification to find how little I had to retract from the teaching laid down so long ago. Some of the doctrines then thought novel have been widely adopted, the remedies I recommended are now regularly prescribed, those I discountenanced have been gradually abandoned, and the only statements I made which provoked opposition have become generally accepted." Having read these prefatory remarks, we were led to expect that this book would be such as to cast all other works on the Throat into the shade; that, like the remedies which the author recommended, this would be *the* book, regularly prescribed. Yet what do we find? From p. 106 to the end, we have

the different diseases of the larynx and pharynx professedly described, but after much circumlocution and frequent repetition—which, perhaps, the author would fain have removed by the introduction of the personal pronoun—we find but scanty information. Treatment is generally ignored. No doubt, in the earlier part of the book a chapter is devoted to it, but this mainly consists in an enumeration of a few therapeutic agents, without any attempt at classification, followed by an account of some of the methods of applying topical remedies. The uneven manner in which the whole subject is treated may be gathered from the fact that four pages are given up to *aconite*, two pages to the discussion of the different parts included in the term “*Throat*” by ancients and moderns, whilst the removal of laryngeal growths is dismissed with twelve lines. For further information we are referred to “my lessons on laryngoscopy.” *Aconite* seems to have been thus specially favoured, for, “from the result of many hundred cases, I ventured strongly to recommend it to the profession in the first edition of this work, since which date it has grown in favour.”

In our attempt to deal candidly with Dr. Prosser James’ book, we have given the reader some extracts from it. We can hardly hope that, in spite of the author’s strong recommendations in its favour, he will hereafter be able to say as much for it as he has done for *aconite* in the paragraph we have quoted above.

*RECENT WORKS ISSUED BY THE NEW
SYDENHAM SOCIETY.*

The Medical Digest. By RICHARD NEALE, M.D. London:
H. K. Lewis. 1877.

THIS work, admittedly an incomplete one, has been published by the New Sydenham Society, apparently to satisfy, in part, one of the original objects proposed by the Society—viz., the preparation of bibliographical indices.

From an examination of its scope and plan of execution, as well as from some personal use of its references, we believe that it affords valuable aid to the student or author desirous of working up the literature of almost any subject of practical or scientific interest which has been discussed within the past forty years. The compilation of this complex volume, although spread over a period of thirty years, must have cost the author an infinite

amount of persevering toil; and with the help of the full and comprehensive index (occupying more than 70 pages), there is no difficulty in finding the clue to any subject the reader is in quest of. The printing, which must have been a very troublesome task, has been admirably executed, and we have reason to believe that comparatively few inaccuracies will be detected in the immense array of figures.

Atlas of Pathology. First Fasciculus. Diseases of the Kidneys.
Plates I. to IV. London: H. K. Lewis. 1878.

WITH the exception of a proposed occasional fasciculus, the issue of the splendid Atlas of Skin Diseases was suspended in 1875 by the Council of the New Sydenham Society, and its place appears to have been taken by this new series of pathological drawings. The present Fasciculus has been prepared under the direction of a very competent committee, and the drawings have been selected from the rich materials collected in the museums of some of the leading London hospitals. In these four plates 26 figures are given which illustrate a variety of morbid changes in the kidney. Plate III. (8 figures) is entirely devoted to "Granular Kidney." Some of the drawings are excellent, in others the colouring is somewhat inartistic; but it is perhaps scarcely fair to criticise too closely until the issue of the next Fasciculus, which will complete the Diseases of the Kidneys, and will furnish a short *résumé* of the present state of knowledge of renal pathology. It is, we think, a pity to select any drawings of which no clinical history is furnished; while, on the other hand, in some cases a mass of tedious clinical detail is given, which might with advantage have been much curtailed.

Bibliotheca Therapeutica; or, Bibliography of Therapeutics, chiefly in reference to articles of the Materia Medica. By E. J. WARING, M.D. Vol. I. London: H. K. Lewis. 1878.

THIS work, like Dr. Neale's Digest, is but a fragment of a larger and more ambitious work which the author commenced many years ago, and has since abandoned. The nature of the book scarcely admits of a review, and its use will necessarily be limited to the comparatively few who are interested in literary research. But when we remember that no work treating specially of medical bibliography has issued from the British press since 1835, the New Sydenham Society may be congratulated upon having under-

taken the publication of Dr. Waring's important contribution to an interesting and much-neglected branch of medical literature. Some idea of the industry of the compiler may be gathered from the fact that he has collated in this volume upwards of 10,000 works relating to therapeutics and articles of the *materia medica*, and he has enlivened this dry catalogue by appending, in many instances, valuable critical, historical, and practical notes.

Clinical Manual for the Study of Medical Cases. Edited by JAMES FINLAYSON, M.D. London: Smith, Elder, & Co. 1878. 8vo. Pp. 584.

ANY work which aims at facilitating the study of disease at the bedside, and which fairly succeeds in its design, cannot fail to become popular amongst medical students. We venture, therefore, to predict for this Glasgow "Clinical Manual" a large measure of success.

No strict nosological plan or arrangement of subjects has been followed in the compilation of this book, which consists of a series of essays by well-known clinical teachers in the medical schools of Glasgow and Aberdeen. Dr. W. T. Gairdner writes on the "Physiognomy of Disease." The editor, Dr. James Finlayson, contributes chapters on "Case-taking," and on "Symptoms of Disorder in the various Systems." Dr. Alexander Robertson has a good practical chapter on "Insanity"—a sadly-neglected subject in medical education. Dr. William Stephenson treats of "Disorders of the Female Organs;" Dr. Samson Gemmell, of the "Sphygmograph" and of the "Physical Examination of the Chest and Abdomen;" and Dr. Joseph Coats, of the "Examination of the Fauces, Larynx, and Nares." The last-named contributor also gives a practical account of the "Method of Performing *Post-mortem* Examinations." The average excellence of the articles reaches a very fair standard, and the value of the work is much enhanced by numerous carefully executed illustrations, and by a very full list of English works referred to in the various chapters of the book as suitable for consultation.

The size of the "Manual" is very convenient, and the manner in which it is published reflects the greatest credit on the eminent firm of Smith, Elder, & Company.

PART III.

HALF-YEARLY REPORTS.

REPORT ON SURGERY.

By WILLIAM THOMSON, A.B., M.D., F.R.C.S.; Surgeon to the Richmond, Whitworth, and Hardwicke Hospitals; and Member of the Surgical Court of Examiners, Royal College of Surgeons.

THE TREATMENT OF ERYSIPELAS.

THE treatment of erysipelas by the subcutaneous injection of carbolic acid solutions, as proposed by Hueter, of Greifswald, does not give much hope to the surgeon. A description of the process is contributed (*Berliner klinische Wochenschrift*, 24, 25, 1878, and *Lon. Med. Rec.*, Oct. 15, 1878). Having satisfied himself that bacteria are the carriers of erysipelatous inflammations of the skin, the rash was first treated with tar ointment, with very good results; but this plan was soon superseded by that of injecting subcutaneously, near the margin of the erysipelatous rash, a three per cent. solution of carbolic acid. When several injections were made, the morbid process passed between the seats of injection in narrow red streaks, which soon coalesced and surrounded well-marked islets of pale and unaffected skin. It was soon made out that, in order to obtain a perfectly successful result, every portion of the affected tissue should be subjected to the influence of this agent. Such a plan would be attended with much difficulty, considering the dangers of carbolic acid poisoning, and the pain of numerous injections. In order to obtain success, it would be necessary to establish the diagnosis of erysipelas as early as possible. The initial symptoms of traumatic erysipelas should be carefully looked for, and attention be paid at once to rigors, nausea, vomiting, and any change in the patient's general condition. Rapidity in the appearance and extension of the erysipelatous rash bears a direct proportion to the severity of the initial symptoms. If there have been severe rigors, much prostration, and frequent vomiting, and, subsequently, high temperature, an unmistakable erysipelatous rash will

soon be observed near the original lesion. When, on the other hand, the initial symptoms have been mild, the rash will not make its appearance until after an interval of some hours. On the commencement of the rash the original wound usually undergoes a change, the granulating surface presenting small gray patches, or being covered with a well-marked croupous deposit. In cases of this kind a solution of chloride of zinc (5 to 8 per cent.) should be applied to the open surface. The whole of the surrounding erysipelatous region should then be thoroughly saturated with a 3 per cent. solution of carbolic acid, injected subcutaneously. Thus treated, two or three injections should suffice; but, in more advanced cases, five or more applications of the injecting syringe will be required.

FRACTURE OF THE PATELLA.

M. Desprès, of the Hôpital Cochin, reports two cases of repeated fracture of the patella, in one of which the bone had been broken twice and in the other three times. The union in every instance was by means of fibrous tissue. Desprès arrives at the following conclusions (*Progrès Médical*, Sept 21, and *Lon. Med. Rec.*, Nov. 15):—"1. The formation of osseous callus in fractures of the patella is impossible when the articulation is distended by a large effusion, or if the pre-existing adhesions do not maintain the fragments in perfect contact in a limited effusion. This opinion, maintained by M. Guyon, finds a new proof. At the beginning of the year a patient was under observation who had sustained a fracture of the left patella. He was treated by a silicate bandage in the extended position, and osseous union was effected. Before the accident the man had suffered from suppuration in the peri-articular tissues, following an affection of the bone. As a consequence of this, the patella was firmly bound to the condyles by strong fibrous adhesions. In this case also, atrophy of the muscles of the thigh caused the absence of one of the principal causes of separation of the fragments. 2. The formation of fibrous callus may be considered as the definitive mode of healing of fractures of the patella, for in the case reported, after the second fracture, as after the first, the patient could walk without fatigue and without limping, there being no difference in the movements of the two limbs. So again, the fibrous callus resisted more than the bone itself, as is evidenced by the second fracture. What use is there in endeavouring to obtain osseous callus when the fibrous is stronger than the

bony? It is to be believed that the formation of an osseous callus is difficult to obtain, and is more desirable than a short solid fibrous material, the development of which depends on the treatment employed."

Hector C. Cameron (*Glas. Med. Journal*, July, 1878) reports a case in which he cut down upon the fragments and brought them together by sutures. The bone was broken in 1876, but three months afterwards the patient was readmitted, suffering from rupture of the connecting fibrous band. Treatment in the ordinary way only resulted in the fragments remaining so far apart as greatly to weaken the limb. As the patient was very anxious that something more should, if possible, be done, Dr. Cameron exposed the patella under antiseptic spray. The ligament between the fragments was cut away, the osseous surfaces were refreshed, and each was drilled in two places with a common bradawl. Through these holes were passed two stout pieces of silver wire, and all blood having been sponged out of the joint, and efficient drainage been provided for, the two fragments were brought together. The wound healed, and the patient was dismissed with a useful limb, but it was evident that the union was not osseous.

Another case is reported by Mr. Henry Smith in *The Lancet*, page 144. The fracture had occurred fully twelve months before. All adhesions to the condyles were dissected away, and the osseous surfaces were refreshed with the saw. The fragments were approximated by wire sutures, but as the quadriceps extensor impeded their being brought together, the muscle was divided subcutaneously three inches above the joint. In eight weeks the cure was perfect; the patella was freely movable over the condyles; the leg capable of flexion to an angle of 45°.

Professor Cooper, of San Francisco (*Med. Times and Gazette*, 1861, Vol. II., p. 467, and *Lon. Med. Rec.*, Nov. 15, p. 479), states that he has done the operation in many cases of recent fracture with unvarying success, one special element of his success being, he believed, that he always prevented union by first intention by stuffing the incision with charpie, and securing a granulating sore.

Uhde, of the Grand Ducal Hospital in Brunswick, also gives a case of this kind in the *Deutsche med. Wochenschrift*, April 27 (*Lond. Med. Rec.*, Aug. 15, 1878). The connexion between the fragments was ruptured a year after the original accident. The fibrous tissue was removed from the surfaces of the fracture by

the sharp spoon and knife. The fragments were brought together by strong wire suture, and the joint was washed out with a five per cent. carbolic acid solution. The operation was recovered from, but the result as regards the function of the limb is not mentioned.

ABDOMINAL OBSTRUCTION.

The important subject of abdominal obstruction, its diagnosis and treatment, was introduced for discussion at the Bath meeting of the British Medical Association by Mr. Jonathan Hutchinson (*B. M. Journal*, Aug., 1878, p. 305). He pointed out various morbid conditions the symptoms of which may simulate obstruction, and which, passing as recoveries from obstruction, have come to exalt some special method of treatment. The following memoranda are important:—

“Memoranda for Diagnosis.

“1. When *a child* becomes suddenly the subject of symptoms of bowel obstruction, it is probably either intussusception or peritonitis.

“2. When *an elderly person* is the patient, the diagnosis will generally rest between impaction of intestinal contents and malignant disease (stricture or tumour).

“3. In *middle age*, the causes of obstruction may be various; but intussusception and malignant disease, both of them common at the extremes, are now very unusual.

“4. Intussusception cases may be known by the frequent straining, the passage of blood and mucus, the incompleteness of constipation, and the discovery of a sausage-like tumour, either by examination *per anum* or through the abdominal walls.

“5. In intussusception, the parietes usually remain lax, and, there being but little tympanites, it is almost always possible, without much difficulty, to discover the lump (or sausage-like tumour) by manipulation under ether.

“6. Malignant stricture may be suspected when, in an old person, continued abdominal uneasiness and repeated attacks of temporary constipation have preceded the illness. It is to be noted also that the constipation is often not complete.

“7. If a tumour be present and pressing on the bowel, it ought to be discoverable by palpation, under ether, through the abdominal walls or by examination by the anus or vagina, great care being taken not to be misled by scybalous masses.

“8. If repeated attacks of dangerous obstruction have occurred

with long intervals of perfect health, it may be suspected that the patient is the subject of a congenital diverticulum, or has bands of adhesion, or that some part of the intestine is pouched and liable to twist.

“ 9. If, in the early part of a case, the abdomen became distended and hard, it is almost certain that there is peritonitis.

“ 10. If the intestines continue to roll about visibly, it is almost certain that there is no peritonitis. This symptom occurs chiefly in emaciated subjects, with obstruction in the colon of long duration.

“ 11. The tendency to vomit will usually be relative with three conditions and proportionate to them. These are: (1) The nearness of the impediment to the stomach, (2) the tightness of the constriction, and (3) the persistence or otherwise with which food and medicine have been given by the mouth.

“ 12. In cases of obstruction in the colon or rectum, sickness is often wholly absent.

“ 13. Violent retching and bile-vomiting are often more troublesome in cases of gall-stones or renal calculus simulating obstruction than in true conditions of the latter.

“ 14. Fæcal vomiting can occur only when the obstruction is moderately low down. If it happen early in the case, it is a most serious symptom, as implying tightness of constriction.

“ 15. The introduction of the hand into the rectum, as recommended by Simon of Heidelberg, may often furnish useful information.

“ Memoranda for Treatment.

“ 1. In all early stages, and in all acute cases, abstain entirely from giving either food or medicine by the mouth.

“ 2. Use anæsthetics promptly. Put the patient under the full influence of ether; examine the abdomen and rectum carefully before tympanites has concealed the conditions; administer large enemata in the inverted position of body; and, if advisable, practise abdominal taxis. If you do not succeed at first, do it repeatedly.

“ 3. Copious enemata, aided perhaps by the long tube, are advisable in almost all cases, and in most should be frequently repeated.

“ 4. Fluid injections may be sometimes replaced by insufflation of air in cases of invagination, since air finds its way upwards better, and is more easily retained. It is, however, somewhat

dangerous, and has, perhaps, no advantages over injection with the trunk inverted.

“ 5. Insufflation is to be avoided in all cases of suspected stricture, since the air may be forced above the stricture, and there retained.

“ 6. Saline laxatives are admissible in certain cases where impaction of fæces is suspected, and in cases of stricture where fluidity of fæces is advisable.

“ 7. Opium (or morphia) must be used in proportion to the pain which the patient suffers. It should be administered by the rectum or hypodermically, and should be combined with belladonna. If there be not much pain or shock, it is better avoided, since it increases constipation, and may mask the symptoms.

“ 8. A full dose of opium administered hypodermically will put a patient in a favourable condition for bearing a prolonged examination under ether, and attempts at abdominal taxis.

“ 9. In cases of uncertain diagnosis, it is better to trust to the chance of spontaneous cure or relief by repeated abdominal taxis, than to resort to exploratory operation; or, in desperate cases, iliac enterotomy should be done. Operations for the formation of an artificial anus in the right or left loin may be performed whenever the diagnosis of incurable obstructive disease in the lower part of the bowel is made.

“ 10. The operation for the formation of an artificial anus through the anterior part of the abdominal wall and into the small intestine should be resorted to only in certain cases of insuperable obstruction, in which the seat of disease is believed to be above the cæcum.

“ 11. In all cases in which the precise seat of disease is doubtful, but the large intestine is suspected, the *right* loin should be preferred. If the colon here be found to be empty, the peritoneum may be cautiously opened and a coil of distended small intestine brought into the wound.

“ 12. My last suggestion as to treatment is one which, speaking as I do in a Medical Section, I feel some delicacy in making. It is, however, I believe, a very important one, and it is this, that cases of mechanical obstruction are really surgical and not medical cases. They require manipulative measures both for diagnosis and for treatment, and they require them early. It is difficult to explain why it has come about that, as a rule, a physician is called in first, and nothing but drug treatment usually adopted in the

early periods; and it is, I am convinced, much to be regretted. The surgeon is but too often asked to see the case only in the last stage, when it is thought that perhaps an operation may be desirable. At this period, the abdomen is distended, and an accurate diagnosis impracticable; but, what is worse, the stage at which abdominal taxis is most hopeful has passed. My remarks do not, of course, apply when the medical attendant possesses the knowledge and exercises the functions of both branches."

Mr. Hutchinson opposes abdominal section until diagnosis can be made much more certain, or the operation much less dangerous. The main argument against it is based on the fact that it is impossible to tell what cases are hopeless. Operations performed at the hernial regions, in search it may be for suspected reduction *en masse*, are of course outside the rule. It refers only to opening the abdomen in the middle with the intent to introduce the hand and search for the obstructed part. In cases of invagination, however, when the included tract is long, and when other measures have been exhausted, abdominal section is probably the best method of treatment.

Cases showing the benefit of belladonna treatment will be found in the *British Medical Journal*, August 31, September 28, and November 16, 1878.

EXCISION OF TUMOURS OF THE PALATE AND FAUCES.

Dr. Foulis, of Glasgow, reports a case in which he removed a tumour from the right half of the soft palate (*British Medical Journal*, October 12, 1878). It was sessile, extending upwards and outwards with a prolongation downwards into the right tonsil. The operation is thus described:—

"Anticipating that blood might flow into the trachea during the operation, I began by performing laryngotomy, tying in a canula into the aperture in the crico-thyroid membrane. This permitted a piece of sponge to be pushed into the upper end of the larynx from the mouth, effectually plugging the larynx. I then cut the cheek open, from the right angle of the mouth to the right angle of the lower jaw, and secured the bleeding points. In the same line of incision, I next divided, with a small saw, the lower jawbone at the angle, and pulled the two rami apart for an inch or more. The right lingual nerve, being thus put on the stretch, was cut across; after which the tumour, which now lay freely exposed, was carefully dissected out. A large vessel at the lower end of the

growth bled freely, but was at once secured. The raw surface was then mopped with solution of chloride of zinc, the sponge-plug was removed from the larynx, and the two rami of the jaw were brought together by two silver-wire sutures, placed in holes drilled in the bone for the purpose. The cheek was closely sewn up, then the tube was withdrawn from the aperture in the crico-thyroid membrane, and, lastly, about one-half of the incision over the larynx was sewn together, the other half being left open to allow the air to escape and to obviate emphysema.

“ On the morning after the operation, when the risk of vomiting was past, a wire-gauze splint was fitted round the chin and lower part of both cheeks, and fastened with tapes, which were tied over the head and behind the neck. This splint was made of wire-gauze (seventeen to the inch), moulded to the shape of the parts. The edges of the splint were made rounded and smooth by simply folding the wire-gauze on itself at the margin with a pair of forceps, any rough points being further masked by a drop of melted gutta-percha applied to them. No other dressing was used, the wire-gauze being smooth and soft enough not to irritate the skin, while it allowed of free ventilation of the surface, and was easily washed and cleaned by the nurse. Recovery took place rapidly. The temperature rose to 100.4° on the second and third days, and after that time fell to and remained at the normal limit. For three weeks or so, the patient was fed with a feeding-bottle and thin oesophageal tube, in order to avoid unnecessary movement of the jaw. On March 16th he went home, able to eat soft food, but still wearing the splint on the lower jaw.* A week later he discarded the splint and returned to his work. The jaw is now (July) quite firm, and mastication and swallowing are perfect and painless.”

* “ The wounds in the cheek and neck had healed kindly. One of the stitches in the jaw-bone having become embedded deeply in the granulation-tissue was left in, and three weeks later (three months after the operation) it was withdrawn through a tiny opening in the skin, which was formed at that time. The wire was quite white and polished, just as it was when put in ; but where it was in contact with the bone it was slightly dull on the surface, as if eroded.”

PART IV.
MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE
COLLEGE OF PHYSICIANS.

SESSION 1878-9.

HENRY H. HEAD, M.D., President.
GEORGE F. DUFFEY, M.D., Honorary Secretary.

Wednesday, December 4th, 1878.

JAMES LITTLE, M.D., Vice-President, in the Chair.

A Case of Disease of the Heart and Aorta. By J. W. MOORE, M.D. Dubl.;
F.K.Q.C.P.; Physician to the Meath Hospital, &c.

Aortic and Mitral Insufficiency; Chronic Endarteritis Deformans; Angina Pectoris.—Robert C., aged fifty years, married, a boot-closer, was admitted to Ward 18 of the Meath Hospital, on March 4, 1878, complaining of cough, difficulty of breathing on slight exertion, impaired vision, swelling about the feet and ankles, and occasional attacks of sharp shooting pain across his chest and down his left arm.

He stated that he had been a strong, hale man until ten or eleven years ago, when he passed through a severe attack of rheumatic fever. Since that time he had become liable to catch cold, and was subject to a cough at times, when he would with difficulty get up a white frothy spit. During the past few years he has suffered from occasional paroxysms of sharp pain in his chest and down his left arm. Any excitement brings on this pain, which is agonising. The pain comes on suddenly, lasts about half an hour, or perhaps only twenty minutes, and leaves him very weak. It attacks him by night as well as by day, causing him to wake with a start, and rendering his sleep very uncertain and broken. When the pain comes on, he has to sit up in bed to catch his breath. The sight became impaired soon after the pain began. His breathing was never very good of late years, but he loses it almost entirely now upon slight

exertion, such as walking up stairs, &c. About last Christmas he noticed some swelling of the feet and puffiness about his ankles. This increases when he has been sitting up for a time, or when he is attacked by the pain in his chest. His appetite has been fairly good, but he is sometimes troubled with flatulence. The water he passes is generally scanty and high-coloured.

As to his family history, we learned that his father died of fever; a brother, of "a pain in his side," at the age of twenty-four. Others of his near relatives succumbed to natural causes. He had been twice married. His first wife was very delicate, her state being for a long time a source of great anxiety to him.

He was a moderate drinker, but used to smoke about a quarter of an ounce of tobacco per diem.

Looking casually at him, one would say he was a fine, strong, healthy man, although of a rather pale complexion. He looks younger than he really is, were it not for his baldness, which is extreme. He seems to be a very intelligent man, is of medium height, and weighs 10 st. 10 lbs. (150 lbs.). At forty years of age, he says, he used to weigh exactly 200 lbs., but since that time he has been gradually losing flesh—particularly during the past nine months. When admitted, his temperature was normal, pulse about 80, and respirations slightly quickened. There was moderate dyspnoea. His tongue was somewhat furred. The face was blanched; there was slight puffiness about the eyes; the left eye seemed weaker than the right, and ran water a little; the right pupil was contracted. The pulse in the radial artery was hard and "hammerstroke" in character. There was visible systolic pulsation in the external carotid, temporal, and femoral arteries, but not to any marked degree in the radial arteries.

Physical examination of the chest yielded the following results:—The thorax was in a measure "barrel-shaped." There was fair expansion of both sides. The percussion note over the upper portions of both lungs in front was hyper-resonant; over the bases of both lungs posteriorly it was more or less dull. Feeble vesicular breathing, with various dry râles were audible in front; muco-crepitus existed behind. There was considerable emphysema of the upper lobes of both lungs, and this led to downward displacement of the liver on the right side and of the heart on the left side. Besides being displaced, the liver seemed to be enlarged, for the hepatic dulness extended some distance below the margin of the ribs and occupied the greater portion of the epigastrium. The area of præcordial dulness began as low as the fourth intercostal space, but extended downwards into the epigastrium and the seventh intercostal space. The cardiac impulse was best marked in this interspace one inch to the left of the nipple line. It was a strong impulse, unaccompanied by thrill. The area of præcordial dulness just described seemed to depend on:

(1) displacement of the whole heart downwards, owing to emphysema of the lung; (2) dilatation of the right ventricle; (3) hypertrophy of the left ventricle. That this last condition was present was evident from—(1) the extension of dulness so far to the left; (2) the situation of the apex so low as the seventh intercostal space and so far to the left—the impulse over a dilated and hypertrophied right ventricle so often observed in emphysema would be towards the right, or into the epigastrium; and (3) the fact that the impulse was towards the left, forcing out the chest wall towards the infra-axillary region.

At the base of the heart, in the right parasternal line and under the sternum, a double murmur—engaging both sounds—was heard. The second sound was clear over the pulmonary area. At the apex a loud systolic murmur existed, which was audible in the axilla and in the left interscapular region.

At this time the urine, although scanty, high-coloured, and turbid from urates, was free from albumen.

The *Diagnosis* was aortic patency, followed by hypertrophy of the left ventricle, mitral insufficiency, chronic bronchial catarrh, pulmonary emphysema, and more or less mechanical congestion of organs. The remote *Prognosis* was very unfavourable.

The *Treatment* consisted in dry-cupping the chest, the administration of diffusible stimulants, digitalis, and liquor strychniæ, the application of a belladonna plaster over the heart, local depletion by leeching over right side of heart, and attention to the state of the bowels and kidneys.

The following notes were taken by Mr. Thomas R. Lingard, the practising pupil in charge of the case:—

March 11th.—Better; chest improved; water more in quantity, and clearer; pain in chest less; basic murmur slightly increased, and heard further down the sternum than is the rule.

March 12th.—Had an attack of the pain last night; is sleeping better, and the pain less frequent; renew the diffusible stimulant mixture.

March 13th.—Improving; basic murmur plainer and coming to the front; mitral murmur less. The first basic murmur is conducted upwards along aorta. Appetite better; left ventricle seems to be increasing in strength. He is passing more water.

March 14th.—The mitral murmur is manifestly systolic; pulse of aortic patency is more evident than it was; lungs and liver not so congested.

March 16th.—Bowels confined; ordered to change his food to potatoes, at his own request.

March 18th.—Not so well; had the pain, which might have been caused by constipation—so ordered two colocynth and hyoscyamus pills. Has stopped the digitalis for the last few days; began taking it again; slept badly last night.

March 19th.—Feels “very lively” this morning; pains gone away,

but cardiac action faster. A double aortic murmur was heard, beyond all question. Apical impulse a little higher up than it was, under 7th rib. The murmur heard in the lower sternal region comes *after* the apical impulse. Pulse, 92. Passes water freely, and has no albuminuria.

March 23rd.—Has a feeling as if food and fluids stop in the epigastrium. This is the case only with *cold* things (from spasm of the muscles of the oesophagus); warm things pass down easily. This brings on an attack of the pain. Double basic bruit heard distinctly; no second sound heard in the neck; has pains from elbow down to little finger.

March 26th.—Getting on very well; has had no pain lately.

The patient left hospital, decidedly relieved, on April 2nd. A fortnight later he was re-admitted, under Dr. Foot's care, with precisely the same train of symptoms, but in an aggravated form. The secondary complications now were—(1) oedema of the lungs, (2) passive hyperæmia of the liver, (3) slight albuminuria, and (4) general anasarca. The oedema of the lungs was evidenced by marked dulness over both bases, general rhonchus, and oedematous sputa with cough. There was considerable anasarca of the lower extremities—the calf measuring fifteen inches. The abdomen was swollen, and he felt “a little swelled all over.” Dr. Foot prescribed for him a diuretic mixture, with infusion of digitalis, a diffusible stimulant mixture, and—should angina come on—a draught composed of two minims of nitrite of amyl in an ounce of camphor water.

At the end of April, Robert C. again came under my care. The following occasional notes, taken by Mr. Alexander B. M'Kee, the practising pupil now in charge of the patient, will illustrate the future progress of the case:—

May 7th.—Passed a bad night; was kept awake by breathlessness and pain in the epigastric region, so that he did not sleep three hours.

May 10th.—To be dry-cupped over chest to relieve dyspnœa.

May 11th.—Dyspnœa greatly relieved by cupping; passed a good night.

May 13th (Monday).—Complains of diarrhœa coming on on Saturday; he had four motions yesterday. His urine was very scanty yesterday, owing probably to the liquid motions from the bowels. He notices that his breathing is freer after making a good deal of water; when an attack of dyspnœa comes on he attempts to make water, and if he succeeds he is greatly relieved; if not, the difficulty of breathing continues. Mr. White gave him two ounces of gin, which relieved dyspnœa.

May 14th.—Diarrhœa has ceased. Passed a good night. He finds the aromatic spirit of ammonia, which was added to his bottle yesterday, an improvement (relieving dyspnœa). Swelling of right leg is increased.

May 20th (Monday).—Passed bad nights on Saturday and Sunday, being greatly troubled by dyspnœa. On Saturday morning complained

of pain in kidneys, and was passing a small quantity of high-coloured urine. A "French blister," or mustard leaf (six inches square) was put on over the kidneys the same night, and left on for twenty minutes. This morning he complains of pains in his limbs, resembling rheumatism, and the right foot and leg are much more swollen. His appetite has been failing for the last few days, his bowels are confined, and he is greatly troubled with wind. Complains of shooting pains from right to left across abdomen, for which last night he got a mustard poultice. Ordered this draught:—

R.—Tinct. Rhei ʒiii.
 Tinct. Lavandulæ Co.
 Tinct. Sennæ aa. . . . ʒi.
 Aquæ Menth. Pip. ad. ʒiss.

May 21st.—"Put in the best night's rest since coming in." Took the draught last night and had two easy motions; thinks so much of draught that he would like to have the prescription. Leg is less swollen.

Ordered a cardiac mixture.

May 25th.—Anasarca is spreading—pitting generally over the back. Heart as before. Loud rhonchus over base of apex.

June 7th.—Increased anasarca of lumbar region; increased dyspnoea, so that he had to sit up all night in bed; ordered two leeches over sternum.

June 8th.—The leeches have given him great relief, although they weakened him at first.

He left hospital on June 13, 1878, without any decided amelioration in his symptoms.

He was re-admitted for the third time on August 10. His state was

catarrh). The entire of the thoracic aorta was atheromatous—the pathological appearances varying from those of an early stage of endarteritis deformans to those of calcareous deposit.

As to the *Heart*, which presented a good example of *cor bovinum*, there was moderate hypertrophy with great dilatation of the left ventricle. There was slight roughening of the aortic valves, which were to some extent incompetent. The mitral valve was dilated, freely admitting three fingers. The anterior curtain and chordæ tendinæ showed traces of deposit. The left auricle was dilated. The right ventricle was largely dilated and somewhat hypertrophied. The tricuspid valve was very wide. There was no calcification of the coronary arteries.

The liver was very hard, the centres of the acini rather pale (chronic atrophy from passive congestion). There were traces of perihepatitis. The spleen was hyperæmic, and its capsule was thickened (perisplenitis). The kidneys were apparently healthy in structure, but there was passive congestion.

Remarks.—The two points which seem to be of most interest in the foregoing clinical record are—(1) the probable sequence of pathological events, and (2) the peculiar attacks of angina pectoris.

(1) It is likely that the attack of rheumatic fever, which the patient had passed through some years before death, was complicated by pericarditis and endocarditis; that the latter condition had led to aortic incompetency and consequent hypertrophy of the left ventricle; that the atheromatous degeneration in the aorta, perhaps, resulted from the to-and-fro movements of strong blood-currents—a strong systolic current being driven up the aorta during the contraction of the hypertrophied left ventricle, and a “back-wash” accompanying the incomplete closure of the aortic valves. As the aortic valves, however, were themselves but slightly diseased, the atheromatous disease in the aorta may have been the first of a series of pathological events, and have led to regurgitation through them. The insufficiency of the mitral valve clearly arose when the left ventricle became unequal to the task of emptying itself, and was, no doubt, coincident with the occurrence of dilatation of that chamber. Mechanical congestion of organs supervened on the mitral lesion.

(2) The etiology of angina pectoris may be regarded as still a *questio vexata*. Dr. Hayden, in his elaborate work on “Diseases of the Heart and of the Aorta” (p. 1040), illustrates the unsettled state of opinion with reference to the pathology of the affection by the following summary of the views held as to its cause:—

(a.) Spasm (distension); Heberden.

(b.) Spasm of the heart; Macbride, Latham.

(c.) Calcification of the coronary arteries; Jenner, Parry.

(d.) Spasm of the heart, pressure of the cardiac nerves against the coronary arteries; Howard Home.

(e.) Neuralgia, located in the pneumogastric, sympathetic, and brachial nerves; Desportes, Laennec, Piorry, Flint, Jaccoud.

(f.) Nervous and spasmodic affection arising from gout; Butter, MacQueen, Johnston, and Blackall.

(g.) Hyperæsthesia of the cardiac plexus; Romberg (on the authority of Niemeyer).

(h.) Hyperkinesis, with hyperæsthesia; Bamberger (on the authority of Niemeyer).

Dr. Hayden himself believes that angina is essentially a neurosis, engaging primarily, in most instances, the cardiac plexus, whence it is reflected through the sympathetic to the spinal nerves, and occasionally through the pneumogastric to the œsophageal plexus. It is, in short, a cardialgia of the most aggravated character, which may or may not be associated with organic disease of the heart. But, in the great majority of cases, it is so associated. Of 19 cases of angina pectoris, collected from the "Transactions of the Pathological Society of London," by Dr. J. W. Ogle, the coronary arteries were calcified in 12, the heart was fatty in 12, the aorta was atheromatous in 10, and valvular disease existed in 6 instances. Grave structural changes of the aortic and cardiac plexuses have been found by Lancereaux.

The presence of atheromatous disease in the aorta, in addition to degeneration of the tissues of the heart, in many cases where this terrible group of symptoms has been observed during life, can scarcely be accidental. Whatever may be the cause of angina, it cannot be doubted that the accompanying neuralgias referred to the left shoulder, arm, and side, depend on the relations of the cardiac plexus through the inferior cervical ganglion of the sympathetic with the brachial plexus and nerves of Wrisberg. The entanglement of nerve-fibres from the cardiac plexus in the inflamed endocardium and intima of the aorta may be looked upon with some probability as one exciting cause, at all events, of the distressing symptoms of angina.

Dr. NIXON, in reference to the cause of the mitral murmur, said he understood there was no organic lesion of the mitral valve, but that it was simply due to over-distension of the ventricle and incompetence of the curtains of the valve. Flint says that in aortic patency with great over-distension of the ventricle a presystolic murmur may be developed from premature closure of the segments of the mitral valve obstructing the passage of blood from the auricle into the ventricle, but he did not think this view had been proved. With respect to treatment, he (Dr. Nixon) did not give digitalis in cases of aortic patency, where he found evidence of great over-distension of the left ventricle. According to Ackermann, the two results which followed the administration of digitalis were—great increase of arterial tension arising from contraction of the

muscular fibres of the arterioles, and prolongation of the diastole of the heart. Anything favouring production of those two conditions would be injurious in cases of aortic patency, for they would lead to increased pressure on the interior of the left ventricle which, where there was evidence of great dilatation, was undergoing degeneration. In the early stages of aortic patency, where there was defective hypertrophy of compensation, digitalis might be administered to ensure a more vigorous contraction of the ventricle. But once they had evidence of great degeneration in the fibres of the ventricle, and consequent dilatation of its cavity, treatment by digitalis was, he thought, contraindicated. As to the sounds at the mitral orifice, Niemeyer makes an important observation—namely, that in cases where mitral murmur was developed in the dilatation of the ventricle following aortic patency, there was a prior resolution of the first sound at the apex of the heart, the valves being closed prematurely.

DR. GRIMSHAW could not agree with the observations of the last speaker as to the use of digitalis. He thought that when the ventricle was apparently losing power and dilatation of the auricle was occurring, the action of digitalis was more likely to be useful than in cases where the ventricle had great power. Dr. Moore had not told them whether the tension of the vessels increased or decreased in the case. In some cases the tension was very great; but dilatation of the heart set in with debility of the muscular tissues of the heart, and the tension then often fell. That was demonstrated by sphygmographic tracings. Angina pectoris, in Dr. Moore's case, was, he thought, caused by the temporary distension of the cavities of the heart, but in cases of distension of the heart it frequently occurred from other causes also. In the present case the condition of the lungs lent force to the view that angina was caused by the increased distension putting an extra strain on the walls of the cavities of the heart.

DR. HENRY KENNEDY did not think they had yet arrived at the exact action of digitalis with respect to angina. He mentioned a case which had recently been under his care. There was aortic disease, and possibly aneurism, and the symptoms were at times very like those of angina pectoris. As to the cause of the series of symptoms observed in angina pectoris, his impression was that they were caused by spasms of the heart aggravated by disease of the coronary artery. He had seen attacks of angina pectoris of the most marked character associated with *post mortem* evidence of fatty degeneration of the heart and perfectly healthy valves.

The CHAIRMAN asked Dr. Moore did he think it possible that the double murmur was due to the disease in the first portion of the aorta? Looking at the valves as they appeared, it seemed hard to believe that there was any incompetency in them. They appeared to be tolerably healthy. The doctrine Dr. Moore had put forward—that the endarteritis

deformans was due to the violent shock inflicted on the first portion of the aorta by the contraction of the left ventricle—did not commend itself to his mind. He was not aware that it was established in these cases. On examining the portions of the aorta beyond the part that could be affected by the contractions of the left ventricle, the same atheromatous change was marked. His impression was that the hypertrophy of the ventricle was sequential to the general arterial disease, and not to the disease of the aorta itself. He therefore asked Dr. Moore did he think it possible that the double murmur was propagated from the first portion of the aorta, and was not due to valvular mischief?

DR. J. W. MOORE, in reply, said that when the patient was first admitted many of the physical signs and symptoms were simply those of mitral regurgitation; but they were led to the diagnosis of coincident aortic valve disease by certain physical signs: one was the double basic murmur, which was quite distinct from the apex murmur referred to the mitral orifice; and the other was the very vigorous cardiac impulse and manifest hypertrophy of the left ventricle. He therefore believed that the mitral lesion was secondary, and depended on the comparative inability of the left ventricle to propel the blood into the arterial system. It was to restore the power of the left ventricle that he administered digitalis; and he thought the result was satisfactory whether he was right in the first instance or not. The physical signs of aortic patency returned in full vigour, the pulse at the wrist became "hammer stroke" in character, and the contraction of the ventricle was now apparently satisfactorily accomplished. There was also a manifest relief of the tendency to passive congestion of organs. Dr. Nixon mentioned two objections to the use of digitalis—namely, that it produced increased arterial tension and increased length of diastole, both of which would be injurious if there was degeneration of the fibres of the left ventricle. But he (Dr. Moore) was at a loss to understand how increased arterial tension could occur in such a case of aortic valve disease as the present, until the digitalis had effected the object for which it had been given. There was no evidence in the present case of extreme degeneracy of the fibres of the left ventricle. The ventricle seemed to have been over-distended without any marked degeneration of fibre; and he hoped by the use of digitalis to restore its power of propelling the blood into the aorta. Dr. Grimshaw's theory of angina pectoris was applicable to the present case, for the attacks disappeared *pari passu* with the apparent recovery of the strength of the left ventricle. He was almost a convert to the Chairman's view of the pre-existence of atheromatous disease of the aorta. Certainly the great extent to which that disease prevailed throughout the aorta pointed to it as being the prior lesion.

Notes on the Treatment of Chorea. By THOMAS HAYDEN, F.K.Q.C.P.;
Physician to the Mater Misericordiæ Hospital.

In the unavoidable absence of the author, Dr. Duffey, Honorary Secretary, read this paper. [It will be found at p. 7.]

DR. M'VEAGH said that in the children's hospital to which he was attached he had seen many cases of chorea. Nearly all of them were due to fright, occasioned by the drunken quarrels of fathers and mothers, in which the poor little children tried to interfere. The children who suffered from it were mostly between eight and twelve years of age. In very young children the disease was rarely met with; and in most of the cases in his hospital they had very little cardiac complications. The treatment he had adopted was generally similar to what Dr. Hayden recommended. They always used the shower-bath, as recommended by Sir Henry Marsh, with hypophosphites and cod-liver oil, and kept the children at rest. At night he gave a small dose of chloral. Bromide of potassium he had not found successful. The solution of phosphorus and strychnia recommended by Dr. Hayden he had never tried. When such children as he mentioned were removed from their miserable homes of dirt and filth, and given good diet, they generally got better. During the last seven years he had had about 200 cases of chorea in his hospital, and they all went out cured. He believed nutritive treatment and rest to be of more benefit than anything else.

DR. HENRY KENNEDY referred to the treatment of chorea by purgatives. He had seen as great and positive results from purgative treatment as from any other. He had seen a great many cases in which tonics and nutritive means entirely failed until after purgatives had been put into use. They were, therefore, likely to go astray if they did not commence with purgatives. He believed that a great number of cases of this disease arose from lodgments in the bowels. He could not otherwise explain the experience he had had, which was very strongly in favour of purgatives. Belladonna also had been used with success; and the dose of extract of belladonna that children would bear was extraordinary. In the majority of cases the treatment by the means he had mentioned succeeded in twenty days, whereas in the cases mentioned by Dr. Hayden the treatment had extended over a much longer period of time. He had also used hemlock, which was perfectly safe, could be administered in a palatable form, and, if persisted in, would cure many cases. The extract of conium, mixed with syrup and cinnamon water, he had found was easily taken by children, and in several cases in which he had used that mixture it had also brought away worms.

DR. WALTER SMITH observed that Dr. J. Harley had stated as the result of his inquiries that extract of hemlock never contained more than one per cent. of conia, and was in his opinion a most worthless preparation.

DR. DUFFEY said that in reading Dr. Hayden's paper he had been struck at the length of time the cases had been under treatment—forty-seven, ninety-one, and seventy-seven days respectively; and it appeared to him that the influence of an important factor in the treatment of chorea—viz., rest and good diet, had not perhaps been sufficiently dwelt upon. It had been shown that cases of chorea got well as rapidly under such simple expectant treatment as when treated by active medicinal agents. Dr. H. Kennedy's successful mode of treatment with the extract of conium corroborated, he thought, this fact; as from the way in which the extract of conium was prepared—viz., by heat, the volatile alkaloid conia, on which the effect of the drug depended, must necessarily be volatilised, and consequently the preparation be almost inert.

DR. H. KENNEDY explained there was ample testimony besides his in favour of purgatives. As to the extract of conium, it had produced in his hands exactly the same symptoms that Dr. Harley had obtained from the *succus*. The objection to giving the latter to children was that it contained a quantity of spirit. He did not mean to say that the extract was a first-rate preparation; still the ordinary dose of it produced exactly the same symptoms as the most powerful preparations of hemlock did even with adults. Individuals had told him that after using it they could hardly raise their eyelids. It produced an anodyne effect, mitigated whooping cough in twenty-four or thirty-six hours, and independent of that had a tonic effect. It would even fatten a child, bring colour into its cheek, and allay spasms, besides bring away worms. The ordinary dose he gave an adult was twelve grains of the extract.

The CHAIRMAN said it was only fair to reinforce Dr. Kennedy's observations by stating a fact known to many members of the Society—namely, that Dr. Hudson, late President of that Society, clung with the utmost tenacity to the effects of extract of conium in phthisis and other diseases; and as a clinical observer he was equal, if not superior, to Dr. Harley. Therefore Dr. Kennedy was not alone in his defence of this classic and now almost forgotten drug.

DR. TAYLOR had seen a great many cases of chorea, and spoke of the value of treatment by purgatives. The most marked cases he had met with had been connected with great constipation.

The debate was then adjourned (Dr. Hayden being absent) until the next meeting of the Society.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

FORTY-FIRST ANNUAL SESSION.

EDWARD B. SINCLAIR, M.D., President.
WILLIAM ROE, M.D., Honorary Secretary.

Saturday, November 16th, 1878.

THOMAS DARBY (outgoing President) in the Chair.

Clinical Report of 752 Cases of Forceps Delivery in Hospital Practice. By
GEORGE JOHNSTON, M.D., F.Q.K.C.P., F.R.G.S.; ex-Master of the
Rotunda Lying-in Hospital, Dublin.

As I have been for many years an advocate for the more frequent use of the forceps in assisting delivery, I beg leave to say that I became so from the circumstance of having witnessed on many occasions the direful results arising from allowing the labour to continue for hours unrelieved—in fact left, as we were instructed, *to depend upon the efforts of nature*. The painful effects of such a system became frequently evident in numerous instances of inflammation and sloughing of the vagina, ruptured uterus, and other accidents, too often terminating in fatal consequences.

I wish, therefore, to lay before you a *resumé* of the results of the practice which was adopted during the seven years of my mastership of the Rotunda Lying-in Hospital, from November, 1868, to November, 1875; but before doing so, if I would not be considered prolix, I beg leave to quote the observations made on the subject in my Clinical Report for 1872:—

“Having now for some time closely watched the process of labour, and carefully considered all the circumstances attendant upon the descent of the foetal head through the pelvis, the injurious effects produced by its long pressure on the soft parts, and in cases where the liquor amnii has escaped at the commencement of labour, the danger that arises from the head pressing on the expanded cervix uteri before the os is fully dilated, we have come to the conclusion, and our established rule is, that so long as nature is able to effect its purpose without prejudice

to the constitution of the patient, danger to the soft parts, or the life of the child, we are in duty bound to allow the course of labour to proceed. But as soon as we find the natural efforts are beginning to fail, and after having tried the milder means for relaxing the parts, or stimulating the uterus to increased action, and the desired effects not being produced, we consider we are justified in adopting prompter measures, and by our timely assistance relieve the sufferer from her distress and danger, and her offspring from an imminent death.

“Why, may I ask, should we permit a fellow-creature to undergo hours of torture when we have the means of relieving her within our reach? Why should she be allowed to waste her strength and incur the risks consequent upon the long pressure of the head on the soft parts, the tendency to inflammation, and sloughing of the vagina, or the danger of rupture of the uterus—not to speak of the poisonous miasm that emanates from an inflammatory state of the passages, the result of tedious labour, and which is one of the fertile causes of puerperal fever and all its direful effects, attributed by some to the influence of being confined in a large maternity, and not to its proper source—i.e., the labour being allowed to continue till inflammatory symptoms appear, and the patient worn out by the fruitless efforts of the labour pains and the evils consequent therefrom?”

“The more we consider the benefits arising from timely interference, and the good results which follow it, the more are we induced to pursue the system we have adopted, and to inculcate and impress on the minds of those we are instructing the advantages to be gained by such practice, both in saving the life of the child, as well as securing the greater safety of the mother. At the same time we do not forget to point out that, although the forceps in the hands of the skilful operator is a perfectly safe and innocuous instrument when used cautiously and with due regard to the internal conformation of the pelvis, on the contrary it becomes the very reverse when its use is attempted by those not thoroughly acquainted with the mechanism of parturition, or who have not acquired that sensitiveness of touch which is so essential to the obstetrician. That although to the looker-on their application may appear simple and easy of accomplishment, still that the greatest care and caution is required in the mode of their introduction, the accuracy of their application, and, eventually, in the method of extraction.”

In order to show clearly the result of the practice, the accompanying table has been framed, by which it will be seen that during the seven years it was deemed prudent to employ the forceps in 752 cases out of 7,862, total delivered, exclusive of abortions—average, 1 in 10½—554 of which were primiparæ and 198 pluriparæ.

TABLE NO. I.

Showing General Results of all the Cases delivered by the Forceps.

	No. of Pregnancy		CHILDREN										Result to Mother		Death of Mothers Average
	First	Subsequent	Total		Alive at Birth		Dead at Birth		Lived		Died		Recovered	Died	
			M.	F.	M.	F.	M.	F.	M.	F.	M.	F.			
Primiparae	554	—	335	219	316	214	19	5	289	199	27	15	506	48	1 in 11½
Pluriparae	—	198	111	87	93	75	18	12	83	67	10	8	188	10	1 in 19½
Total	752		446	306	409	289	37	17	372	266	37	23	694	58	1 in nearly 13

Of the 554 *primiparae*, 335 were delivered of male children, and 219 of female.

Of the 335 male children, 316 were born alive, and 19 were dead at birth. Four of these being dead and putrid, leaves 15 for which the practice may be considered accountable. Average, 1 in 22½. Although I say “for which the practice may be accountable,” I do not mean that the forceps being used was; in all those cases, the cause of the death of the child, as it is evident that many of them would, if left to the natural efforts, have perished from the long-continued action of the uterus or other causes.

Of the 316 males born alive, 289 lived, and 27 died, or 1 in 11½.

Of the 219 female children, 214 were born alive, and 5 were dead at birth; average, 1 in 43½.

Of the 214 born alive, 200 lived, and 14 died, or 1 in 15¾.

Mothers.—Of the 554 *primiparae*, 506 recovered and 48 died, or 1 in 11½. Of these 48, 28 were suffering from great mental anxiety, attributable principally to seduction, 12 of whom died of peritonitis;^a 4 of pyæmia;^b 1, phthisis;^c 2, acute bronchitis; 1, acute laryngitis; 1, clot in heart;^d 1, nephritic disease;^e 1, gastro-enteritis;^f 1, typhoid fever;^g 1, scarlatina;^h 1, rupture of the uterus;ⁱ 2, of extreme fretting—no pathological appearance at P. M. examination.

^a One attempted suicide before coming in. One commenced drinking whiskey on finding she was pregnant. In this case there was sloughing of the perinæum.

^b One came from the Island of Jersey to avoid publicity.

^c Phlebitis followed.

^d Died suddenly on second day. P. M. examination—clot found in left ventricle of heart.

^e Kidney found greatly diseased.

^f Admitted with gastro-enteritis.

^g Admitted with fever.

^h Appeared immediately; died fourth day.

ⁱ Some time in labour before admission.

Of the remaining 20, 4 died of peritonitis, 3 of pyæmia, 2 of sloughing (1 where there was complete separation of the cervix from the body of the uterus, and 1 a case of sloughing of the vagina, caused by too frequent examination), 2 of gastro-enteritis, 5 convulsions, 1 meningitis, 2 nephritis, and 1 from shock.

Pluriparæ.—Of the 198 pluriparæ, 111 were delivered of male children, and 87 female.

Of the 111 male children, 93 were born alive and 18 were dead at birth, 3 of these being dead and putrid, leaving 15, or 1 in $7\frac{6}{13}$. Of the 93 males which were born alive, 83 lived and 10 died, or 1 in $9\frac{1}{31}$. Of the 87 female children, 75 were born alive, and 12 were dead at birth, 1 of these being dead and putrid, leaving 11, or 1 in 8. Of the 75 born alive, 67 lived and 8 died, or 1 in $8\frac{1}{19}$.

Mothers.—Of the 198 pluriparæ, 188 recovered and 10 died, or 1 in $19\frac{4}{5}$. 2 of these were suffering from great distress of mind, having been deserted by their husbands, 1 died of acute bronchitis, the other of acute pleuritis.

Of the remaining 8, 1 her 5th pregnancy, died of peritonitis.

1	„	3rd	„	„	erysipelas.
1	„	2nd	„	„	typhoid fever.
1	„	3rd	„	„	rupture of uterus.
1	„	2nd	„	„	convulsions.
1	„	2nd	„	„	fungoid tumour of uterus.
1	„	4th	„	„	shock.
1	„	5th	„	„	carcinoma uteri.

Thus it will be seen that of the total 752 deliveries with the forceps, 446 male and 306 female children were delivered. Of the 446 male children, 409 were born alive and 37 were dead at birth, 7 of which were putrid, and therefore not counted; the average being 1 in $14\frac{1}{2}$. Of the 409 male children born alive, 372 lived and 37 died, or 1 in $11\frac{2}{7}$. Of the 306 female children, 289 were born alive, 17 were dead at birth, 1 of which was putrid; thus 16 are to be accounted for, or 1 in $19\frac{1}{8}$. Of the 289 born alive, 266 lived and 23 died, or 1 in $12\frac{1}{2}$. The total number of children saved were—Male, 372

„ „ „ Female, 266

638

Total number of children dead at birth—Male, 37, 7 of which were putrid

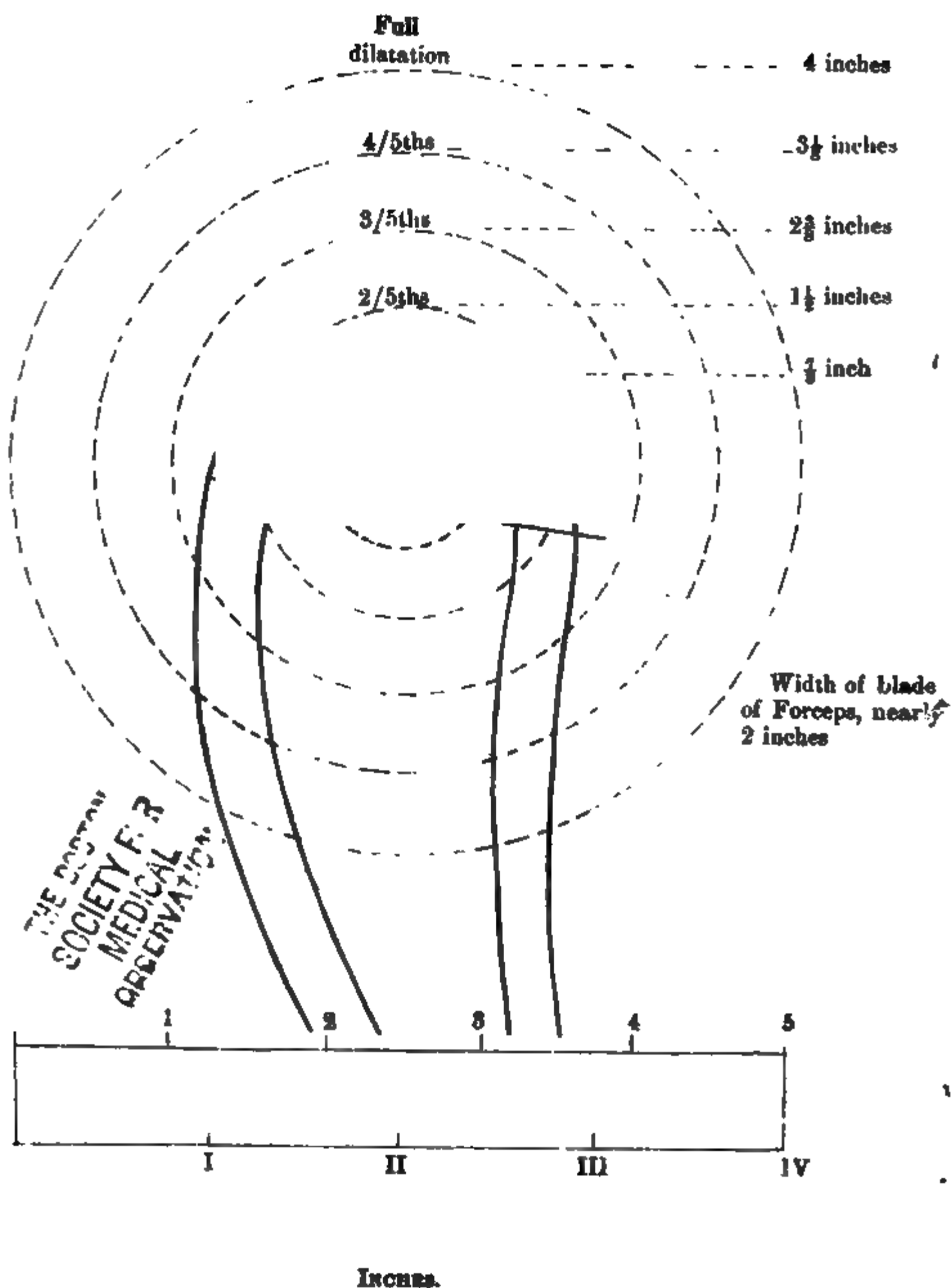
„ „ „ Female, 17, 1 „ was putrid

54, 8 „ were putrid

Total number died—Male, 37

„ Female, 23

60



DEGREES OF DILATATION OF THE OS UTERI.

Children born alive, 698 ; dead at birth, 54, 8 of which were putrid, thus leaving 46, or 1 in $16\frac{1}{2}$; died, 60, 1 in $11\frac{1}{8}$.

Mothers recovered, 694 ; died, 58 ; or nearly 1 in 13.*

DELIVERY BY THE FORCEPS BEFORE THE OS UTERI IS FULLY DILATED.

In speaking of this proceeding I consider it necessary in the first place to state that, although in such cases the os uteri was dilated only to the extent mentioned, it, nevertheless, *must have been dilatable*—i. e., capable of further expansion. Should it be rigid, the usual means for relaxing it must be adopted previous to attempting to operate. But before going further, I must here caution the practitioner that this operation (as in any case where the forceps is used to aid delivery) is not without danger in unskilful hands, by whom it should never be attempted ; but if performed by those who have thoroughly acquired that great delicacy of touch so essentially necessary in the obstetrician, and who have had sufficient experience in the use of the forceps—in their hands, when properly and carefully employed, it is perfectly safe and its use justifiable (not in any manner injuring the soft parts), for it not only in a great measure secures the safety of the mother, but tends materially to the preservation of the life of the child. The former, by obviating the danger produced by prolonged pressure of the foetal head on the maternal soft parts, and all its evil consequences, and the latter, by preventing interference with the placental circulation endangering the life of the child, particularly in cases of early rupture of the membranes.

From March 21st, 1869—when the first case occurred—till November 5th, 1875, out of the 752 cases delivered with the forceps, as already mentioned, there were 169 cases where delivery was thus effected.

For the purpose of giving a clear and precise idea of the amount of dilatation of the os uteri at the time of operation, it is assumed that four inches is the greatest diameter of expansion of the os, or what is called “fully dilated”—i. e., at the time the head is passing through it.

This four inches is divided into five parts, as may be seen by the accompanying diagram.

This division, we consider, gives a more accurate idea of the extent of expansion than the ordinary designation of the degrees—viz., sixpence, one shilling, half a crown, or crown piece.

In order to exemplify the result of the practice, the following three tables, A, B, C, showing the degrees of expansion of the os uteri at the time of operating, the cause of interference, the position of the foetal head, whether above the brim, in the brim, or in the cavity of the pelvis, the

* In four reliable tables of forceps statistics—taken from Drs. Hardy and M'Clinck's reports, Dr. C. Johnson's ward books, Drs. Shekleton and Sinclair's reports, and Dr. Denham's ward books—I find that out of 581 deliveries, there were 70 deaths ; average, 1 in $9\frac{1}{2}$.

result to the children and the same to the mothers, together with the cause of death in the fatal cases, will give a clear and, I trust, a satisfactory view of what I am anxious to explain and establish.*

By Table A it will be seen that there were 59 cases where the os uteri was two-fifths dilated, 44 being primiparæ and 15 pluriparæ.

Of the 44 *primiparæ*, the *cause of interference* was in 36 instances early rupture of the membranes; in 4, the head pressed on the expanded though undilated cervix, particularly the anterior lip, although the membranes were entire, the liquor amnii remaining above the head of the foetus; in 1, prolapse of the funis (the child, a girl, lived); in 2, convulsions (children both male, and dead); in 1, disproportion.

Position of Head.—In 9 instances the head was above the brim at the time of the application of the forceps; in 14, it was in the brim; and in 21, it was in the cavity.

Result to Children.—25 male children were born and 19 female. Of the 25 males, 21 were alive at birth, 1 of whom died on the seventh day of erysipelas, and 3 were still-born, 1 of which was putrid; in 1 the waters had escaped thirty hours; and 1 was a case of convulsions. Of the 19 female children, 17 lived and 2 were still-born, of which 1 was a case of convulsions, and 1 where the waters had escaped 36 hours.

Results to Mothers among Primiparæ.—38 mothers recovered and 6 died, of whom 2 were cases of convulsions, both brought to hospital comatose, having had several fits before admission.

One was a case of gastro-enteritis, from which she was suffering for a week before being admitted.

One was a case of sloughing, attributed to too frequent examination.

One was a case of peritonitis; patient fretting; suspected seduction.

One was a case of great distress of mind from seduction.

At the P. M. examination of these 6 cases, *in none* was the os uteri found to be more fissured than is usually observed after first deliveries.

Of the 15 *pluriparæ* the cause of interference was in 6 instances from early rupture of the membranes; in 4, from pressure of the foetal head on the cervix; in 1, from prolapse of the funis; in 1, from placenta prævia; in 2, from deformity (projection of the sacral-promontory); in 1, from acute laryngitis, where tracheotomy had to be performed and labour induced.

* On looking back to my Clinical Report of 1875, in the Table No. 5, where "the forceps was used in the first stage of labour," page 18, a discrepancy exists with the present Report, which, after a careful perusal of the tables, I find to be incorrect—viz, in the column of the degrees of dilatation of the os uteri, 58 is the total number mentioned where the os was $\frac{2}{5}$ ths dilated, whereas it should be, as in the present Report, 59; 80 as the number where the os was $\frac{3}{5}$ ths dilated should be 71; 33 where the os was $\frac{4}{5}$ ths dilated should be 39; and the number of deaths, 10, should be 9.

TABLE No. II.—Showing the Number of Cases in which Delivery was effected by the Forceps before the Os Uteri was fully Dilated.

A.—WHERE THE OS UTERI WAS $\frac{1}{2}$ DILATED.

	Cause of Interference										Position of the Head when Forceps applied			Result to Children						Result to Mother		Cause of Death								
	Total	Early Rupture of Membranes	Pressure on Anterior Lip	Malposition of Head	Prolapse of Funis	Convulsions	Accidental Hemorrhage	Placenta Previa	Disproportion	Deformity	Laryngitis	Extreme Nervousness, Great Exhaustion	Above the Brim	In the Brim	In the Cavity	Total	Dead Born		Lived		Died	Recovered	Died	Gastro-Enteritis	Sloughing	Peritonitis	Uterine Diphtheria	Disease of Mind		
																	M.	F.	M.	F.									M.	F.
Primiparae	44	36	4	—	1	2	—	—	1	—	—	—	9	14	21	25	19	3	2	21	17	1	—	38	6	1	—	—		
Pluriparae	15	6	4	—	1	—	—	1	—	2	1	—	9	3	3	7	8	—	—	3	7	—	1	15	—	—	—	—		
Total	59	42	8	—	2	2	—	1	1	2	1	—	18	17	24	32	27	7	2	24	24	1	1	53	6	1	—	1		

B.—WHERE THE OS UTERI WAS $\frac{2}{3}$ DILATED.

Primiparae	53	39	10	1	1	1	—	—	1	—	—	—	4	26	23	27	26	4	—	23	22	—	4	51	2	—	1	—
Pluriparae	18	4	3	2	2	—	1	—	1	3	—	2	2	10	6	12	6	2	1	8	5	2	—	18	—	—	—	—
Total	71	43	13	3	3	1	1	—	2	3	—	2	6	36	29	39	32	6	1	31	27	2	4	69	2	—	1	—

C.—WHERE THE OS UTERI WAS $\frac{3}{4}$ DILATED.

Primiparae	26	18	7	—	—	1	—	—	—	—	—	—	3	8	15	19	7	1	—	16	5	2	2	25	1	1	—	—
Pluriparae	13	7	2	—	—	1	2	—	1	—	—	—	1	2	10	8	5	1	3	7	2	—	13	—	—	—	—	
Total	39	25	9	—	—	2	2	—	1	—	—	—	4	10	25	27	12	2	3	23	7	2	2	38	1	1	—	—

TOTAL.

Primiparae	128	93	21	1	2	4	—	—	2	—	—	—	16	48	59	71	52	8	2	60	44	3	6	114	9	1	1	1
Pluriparae	46	17	9	2	3	1	3	1	1	6	1	2	12	15	19	27	19	7	4	18	14	2	1	46	—	—	—	—
Total	169	110	30	3	5	5	3	1	3	6	1	2	28	63	78	98	71	15	6	78	58	5	7	160	9	1	1	1

Position of the Head in the Pelvis.—In 9 instances the head was above the brim ; in 3, in the brim ; in 3, in the cavity.

Results to Children.—7 male children were born and 8 female. Of the 7 male, 3 lived and 4 were still-born, 1 of which was putrid. Of the still-born, 1 was a case of prolapse of the funis ; 1 where the waters had escaped 63 hours, besides which there was projection of the sacral promontory ; 1, a case of acute laryngitis in the seventh month, mentioned above. Of the 8 female children, 7 were alive at birth, 1 of whom died in 17 hours in a case of placenta prævia—7th month, 16th pregnancy.

Result to Mothers.—All recovered.

By Table B you will perceive that there were 71 cases where the os uteri was three-fifths dilated, 53 being primiparæ and 18 pluriparæ. In the 53 *primiparæ*, the cause of interference was—in 39 instances the membranes had ruptured and the waters escaped at the commencement of labour ; in 10, the head pressed on the cervix, the membranes unbroken ; in 1, malposition of the head ; in 1, prolapse of the funis ; in 1, convulsions ; in 1, disproportion.

Position of the Head.—In 4 instances the head was above the brim at the time of operation ; in 26, in the brim ; in 23, in the cavity.

Results to Children of Primiparæ where the Os Uteri was three-fifths dilated.—27 male children were born and 26 female. Of the former, 23 were born alive and lived ; 4 were still-born, 3 being in cases where the membranes had ruptured early ; 1 where the head had pressed on the anterior lip. Of the 26 female children, 22 were born alive and lived, and 4 died, 2 of which were overlain ; 1 child (9 lbs. 4 oz.) died the fourth day in a case suffering from bronchitis before admission—early rupture of membranes ; and 1 child (4 lbs.) premature.

Result to Mothers.—51 recovered ; 2 died—1, a case of gastritis, from which she was suffering for some time previous to her coming in ; 1, a case of uterine diphtheritis, died on third day.*

In none of these cases at P. M. examination was there seen any injury to the os uteri beyond the ordinary fissuring that would take place in first labours.

Of the 18 *pluriparæ*, the cause of interference was—in 4 instances early rupture of the membranes ; in 3, the anterior lip was pressed on by the head ; in 2, malposition of the head ; in 2, prolapse of the funis ;

* P. M. examination—"Peritoneal sac contained some muddy semi-purulent fluid ; slight peritonitis ; right ovary covered with lymph ; both fimbriated extremities of the Fallopian tubes intensely congested ; uterus well contracted ; no metritis, but about four or five small patches of diphtheritic membrane at the cervical portion ; os fissured slightly at both sides, but fissure not extending deep." This woman noted as having been delicate all through pregnancy, hard worked, badly fed, admitted anæmic, and extremely weak. Third morning found pulseless, and died shortly after.

in 1, accidental hæmorrhage; in 1, disproportion; in 3, deformity; in 1, extreme nervousness; in 1, great debility, a case of phthisis.

Position of the Head when the forceps were applied.—In 2 instances it was above the brim; in 10, in the brim; in 6, in the cavity.

Results to Children.—12 male children were born and 6 female. Of the former, 10 were born alive, 2 of which died; 1 weighed 5 lbs. 14 oz., premature, eighth month, died in 15 hours; 1 weighed 4 lbs. 12 oz., seventh month, in the case of phthisis; 2 were dead born; 1, 9 lbs. 3 oz., face presentation; 1, 6 lbs. 6 oz., prolapse of funis. Of the 6 female, 5 lived; 1 was dead at birth, seven months, weighed 6 lbs. 12 oz., in the case of acute laryngitis.

Result to Mothers.—All recovered.

By Table C, *where the os uteri was four-fifths dilated*, it will be seen that the forceps was had recourse to in 39 cases, 26 being primiparæ and 13 pluriparæ.

Of the 26 *primiparæ*, the cause of interference was in 18 cases early rupture of the membranes; in 7, the head pressed on the cervix; in 1, convulsions—brought in comatose.

Position of the Head in the Pelvis.—In 3 instances it was above the brim; in 8, in the brim; in 15, in the cavity.

Result to Children.—19 male and 7 female children were delivered. Of the 19 male, 18 were alive at birth and 1 dead—had been a long time in labour, under care outside; 16 lived and 2 died, in both of which the membranes had ruptured early—one 35 hours, and one 26 hours previously. Of the 7 female, all were born alive, but 5 lived and 2 died. Of the 2 who died, 1 was a case of convulsions, at seven months, died in 8 hours, the mother having had several fits before admission; 1 was where the head pressed on the cervix.

Result to Mother.—25 recovered; 1 died—admitted perfectly comatose, having had four fits of eclampsia before being brought to hospital.

Of the 13 *pluriparæ*, the cause of interference was—in 7 instances early rupture of the membranes; in 2, pressure of the head on the cervix; in 1, convulsions, her second child; in 2, accidental hæmorrhage; in 1, projection of the sacral promontory.

Position of the head in the pelvis at the time of the application of the forceps.—In 1 case it was above the brim; in 2 cases in the brim; in 10 cases in the cavity.

Results to Children.—8 males and 5 females were born. Of the 8 males, 7 lived and 1 was putrid at birth. Of the 5 females, 2 lived and 3 were dead-born, 2 being cases of accidental hæmorrhage; 1 a case of early rupture of membranes.

Result to Mothers.—All recovered.

Thus the total number of *primiparæ* delivered was 123.

The cause of Interference being in 93 instances from early rupture of

membranes; in 21, head pressing on cervix; in 1, malposition of head; in 2, prolapse of the funis; in 4, convulsions; in 2, disproportion.

Position of the Head in the Pelvis.—In 16 cases it was above the brim; in 48, in the brim; in 59, in the cavity.

Results to Children.—71 male and 52 female children were born; of the 71 male children, 8 were dead at birth, 1 of which was putrid, 60 were alive at birth, 3 of whom died; of the 52 female children, 2 were dead at birth, 44 lived and 6 died.

Result to Mother.—114 recovered, and 9 died—viz., 3, convulsions; 2, gastro-enteritis; 1, sloughing of vagina; 1, peritonitis; 1, uterine diphtheritis; 1, distress of mind.

The total number of *pluriparae* delivered was 46.

The cause of Interference was—in 17 cases, early rupture of the membranes; in 9, head pressing on the cervix; in 2, malposition of head; in 3, prolapse of the funis; in 1, convulsions; in 3, accidental hæmorrhage; in 1, placenta prævia; in 1, disproportion; in 6, deformity; in 1, acute laryngitis; in 2, extreme nervous delicacy.

Position of the Head in the Pelvis.—In 12 instances it was above the brim; in 15, in the brim; in 19, in the cavity.

Result to Children.—27 male and 19 female children were delivered. Of the 27 male, 7 were dead at birth, 2 of which were putrid, 18 were alive at birth, 2 of whom died. Of the 19 female children, 4 were dead at birth, 14 were born alive, 1 of whom died.

Result to Mothers.—All recovered.

Thus of 169 deliveries, 160 mothers recovered and 9 died, or 1 in 18½. Of 98 male children delivered, 78 were alive at birth, 15 were dead at birth, 3 of which were putrid, and 5 died. Of the 71 female children delivered, 58 were born alive, 6 were dead at birth, and 7 died.

The instrument we at first employed was the straight form, which at the time was generally used in Dublin, both short and long, according as the head was low down or high in the pelvis; but on very many occasions, particularly in the latter instances, finding them fail, as they slipped from off the head when it was above the brim, or it was at all impacted, we eventually had recourse to Barnes' double-curved forceps, which we have ever since found most satisfactory, being equally easy of introduction, and when applied grasping the foetal head so firmly that we hardly ever failed to deliver with them.

In no instance did the patient sustain any injury of the uterus or vagina from the instrument, either by the passing of the blades or in the process of extraction; laceration of the perinæum sometimes did occur, but not to any serious extent, nor was it attributable to the forceps, as from the precautions always taken in removing the blades, as soon as the head descended so low that the occiput was brought under the arch of the

pubis, and the forehead to bulge the perinæum, we avoided the occurrence of such an accident.

Chloroform was employed in 537 cases of forceps deliveries, in most of which it was pushed to full anæsthesia, and always with satisfactory results—the patients recovering sensibility in a few minutes after its withdrawal—and no unpleasant consequences occurring. We always took the precaution, previous to its administration, of giving a full dose of ergot of rye, in order to guard against *post partum* hæmorrhage.

The length of time occupied in the delivery of the head varied from seven or ten minutes to, in one case, three-quarters of an hour.*

There were 2 cases of sloughing—both primiparæ. The first where there was early rupture of the membranes, the head pressing on the cervix. The usual remedies to relax the os and excite uterine action proving unavailing, delivery of a boy, living, was assisted by the forceps; she died suddenly third day after. *Post mortem* examination showed complete separation of the cervix from the body of the uterus. This was the case which first induced me to adopt this practice.

The second was a case of sloughing of the vagina, caused by the too frequent examination of the patient; 26 hours in labour; the os two-fifths dilated when delivery was effected of a boy, weighing 8 lbs. 6 oz., lived; mother died on 13th day. *Post mortem* examination—os not more fissured than after ordinary labour.

There were 6 cases of sloughing of the perinæum, which occurred to patients in bad health, attributed to distress of mind, being victims of seduction; 3 recovered.

There was no instance where we were obliged to alter the position of the blades after having been applied.

The reasons we considered it necessary to employ the forceps were, as will be seen by the table, when convulsions set in, cases of accidental or unavoidable hæmorrhage, prolapse of the funis, extreme weakness, or in cases where there was no advance although uterine action was frequent, and threatening injury to the soft parts, particularly where the waters had escaped at the commencement of labour, or the life of the child was endangered. We did not allow the labour to be prolonged so far as to produce any of the symptoms indicative of vaginal inflammation, considering it safer to interfere before such should appear.

* B. B., aged thirty-nine or forty, ninth pregnancy; waters had escaped 23 hours before admission; when examined, os found three-fifths dilated; head arrested above brim, owing to an exostosis of left sacro-iliac synchondrosis. After trying stimulating enema without producing any effect, she was given a dose of ergot, put under chloroform, and the forceps were applied with great difficulty, and after a considerable amount of traction and time, a female child was delivered—left frontal bone being greatly depressed for a space of $8\frac{1}{2} \times 1\frac{1}{2}$ inches. It was some time before child could be resuscitated, the depression decreased, and both mother and child went out well on 12th day.

In the after-treatment of almost every case we took the precaution of having the vagina syringed regularly, twice or three times a day, with an antiseptic solution.

There never was any serious injury inflicted on the head of the child by the blades of the forceps; sometimes a slight abrasion on the head occurred, and occasionally we had facial paralysis, but in all cases it passed off in a few days.

From the foregoing statistics, I think it is amply proved that the practice I have recommended is not alone safe and justifiable, but also a great preservative of the lives of both mothers and children.

Saturday, December 7th, 1878.

EDWARD B. SINCLAIR, M.D., President, in the Chair.

THE discussion on Dr. Johnston's "Clinical Report on the Use of the Forceps during Seven Years, from 1868 to 1875," took place.

DR. ATTHILL said:—The practice of the whole kingdom is greatly influenced by the teaching of the Dublin School of Midwifery. When, therefore, a gentleman occupying the position Dr. Johnston did, as being so recently the head of a great clinical midwifery hospital, pronounced a very decided opinion, his practice should be freely and fully discussed in this Society.

In the first instance he wished to say that his (Dr. Atthill's) practice in the Rotunda Hospital did not vary in principle, at least very materially, from Dr. Johnston's. Dr. Johnston used the forceps once in about every 10 cases; he once in about every 15. Dr. Johnston applied them frequently before the os was fully dilated; he also used the forceps before the os was fully dilated, but avoided doing so as far as his power lay. It was unnecessary for him to say a word in defence of the forceps. There was hardly a practitioner who objected to its use; and the hesitation of any who did so arose from that timidity which is begotten by ignorance. The objections of such were fully answered by Dr. Johnston in the commencement of his paper. But though he agreed with what was there said in defence of the forceps, he was not sure that the class of cases which Dr. Johnston described in the paragraph alluded to, were those which formed the mass of the cases upon which he founded his paper. None of them reached the extreme conditions he described, and comparatively few would have done so. Nevertheless, that the great majority of them were properly delivered with the forceps he (Dr. Atthill) had not the slightest doubt. Still there was the risk of running into extremes. There was the danger of encouraging practitioners throughout the country to a too free use of the forceps.

The main feature of the paper was the advocacy of the use of the forceps before the os uteri was fully dilated. That practice was not

altogether new. In certain cases it would be monstrous to leave a woman undelivered because the os had not fully dilated; and even excepting cases in which hæmorrhage, or convulsions, or prolapse of funis occurred, there were many others—in which, after twelve or sixteen hours' labour, the patient becomes exhausted, the pulse rises, and matters evidently become serious, and in which there was no physical difficulty in applying the forceps, or any reason to suppose that the os would not yield—which should be delivered at once. On the other hand, he had seen cases in which serious injury resulted from the too early use of the forceps. He had seen the lips of the os uteri at the very labia during extractions with the forceps. The head was, of course, in the pelvis; the cervix was thick and swollen, and was compressed between the arch of the pubis, anteriorly, and the foetal head. He had seen severe contusions produced by that operation, followed by sloughing, which resulted in death. He had also seen ulcerations of the cervix, but not to an excessive extent. Besides these objections there was another—namely, that great force was sometimes necessary to deliver with the forceps, from the resistance of the cervix. His powers in such cases had been taxed to the uttermost. Again, members of the Society would remember a case which occurred about a year ago in England, in which a junior practitioner was put on his trial for manslaughter, in consequence of death following from the injudicious use of the forceps. In that case the blades had passed through the cervix, which was spread out over the foetal head. The os had been only half dilated. Therefore the forceps should not be applied before full dilatation of the os, without the exercise of great care and judgment.

Dr. Johnston, in his Report of the hospital for the year 1874, at page 69, gave a table of 42 cases in which he applied the forceps before the os was fully dilated. In No. 39 of those cases the patient had been only six hours in labour—in another eight. In four other cases the labour was of but twelve hours' duration. In all these the cause assigned for the use of the forceps before the os was dilated was early rupture of the membranes. He did not mean to say that those cases should not have been delivered with the forceps, but that table was calculated to mislead an inexperienced practitioner.

Next, what bearing had the use of the forceps on infant mortality? It was stated that a considerable number of children were saved by the use of the forceps, but he believed that the impressions on that subject were exaggerated. His own experience was that the use of the forceps before the os was fully dilated did not save a large number of children. They might be born alive in the sense of breathing for a few moments, but he was satisfied that in many cases they did not survive. The number of children that died after the early use of the forceps he believed to be considerable. That was proved from Dr. Johnston's own tables. It was

stated there that out of 316 males born alive, 27 died before leaving the hospital, and that of females 14 died out of 214. He did not exactly know how to go about showing what the relative mortality was, but he would compare Dr. Johnston's tables with those of his predecessors. In Johnston and Sinclair's Midwifery difficult and tedious labours were classed separately. Tedious labours were those exceeding 24 hours—difficult, those in which it was thought necessary to use instruments of any kind. Out of 13,800 patients dealt with by Johnston and Sinclair as occurring in Dr. Shekleton's mastership, there were 247 cases of tedious, and 200 of difficult labour—making 447 in all. These two classes should correspond with Dr. Johnston's forceps cases.

Dr. Johnston dealt with only 7,800 patients, and his proportion of both tedious and difficult cases was 752; therefore, while Dr. Johnston used the forceps once in about every 10 cases, Dr. Shekleton met with only one case of difficult or tedious labour in about every 31 cases. If the forceps save life to the extent claimed for them, we should expect that the infantile mortality in Dr. Johnston's practice should be very much less, but we find that Dr. Shekleton's mortality was 6·9 per cent. of all his cases, against Dr. Johnston's 6·1 per cent.; but this only refers to stillborn children, and I affirm that if we add those children who died within a few days of birth, there would be no gain in favour of Dr. Johnston's practice. Again, during the mastership of Dr. Collins, the mortality of infants was 6·5 per cent. out of 15,800 cases. Dr. Shekleton, out of 13,748 cases, had 6·9 stillborn. Dr. Collins hardly ever used the forceps. Dr. Shekleton used it freely. Dr. Johnston, out of 7,830 cases, lost 6·1. Evidently the saving of children by the use of the forceps was not very great. He (Dr. Atthill) thought the advantage to the mothers was greater, consisting in relief from protracted suffering, still he could not but think that the advantage of the forceps was overrated, and that there was a danger at present of its being too freely used.

DR. MACAN said the question had resolved itself into this—In what cases, before full dilatation of the os, was it right to use the forceps? In what cases was the danger of using the instrument less than the danger the woman would run from protracted labour, and the complications that might thence arise? The danger of bleeding to death might be greater than any that was connected with the use of the forceps before full dilatation. He had saved a woman from bleeding to death by the use of the forceps. There were some practitioners who, although they might refuse to apply a forceps before full dilatation, would not have the slightest hesitation in passing in the hand and turning the child. The use of the forceps took about half an hour, that of the hand about ten minutes. The whole danger depended on the time given to the cervix to distend. They were told to take the greatest care not to include the cervix between the head and the forceps. On the other hand, if the os

were fully dilated, the cervix could not be included. Again, it was wrong to make the mere size of the os the criterion of danger. It was sometimes more dangerous to apply a forceps with a dilatation of two inches than in other cases where the dilatation was only a quarter of an inch. There was a difference, in using the instrument, between the cases of first labours and of women who had borne many children. In the former the cervix was pressed as thin as blotting-paper, and, though the os might be only the size of the tip of the finger, the mere sweeping of the finger round it would dilate it an inch and a half. In the cases of pluriparæ the forefinger could be passed into the cervix a month before labour. If the cervix round the os was extremely thin, the conditions for the use of the forceps were more satisfactory than when the cervix was still very thick, although the os might be somewhat larger. On the whole, if the use of the forceps saved suffering, without increasing mortality, great service was done to the cause of obstetrics.

DR. MORE MADDEN said Dr. Johnston had more than once spoken of this practice as a novel one, yet it was brought forward 125 years ago by Smellie. It afterwards came into general vogue, and practitioners delivered their patients as quickly as they could with the forceps. Then, as usual, a revulsion of feeling ensued, and others began to oppose the practice and to deprecate the digging out of a child untimely from its mother's womb. Dr. Johnston had referred to a diagram in illustration of his paper. It represented the degrees of dilatation which the os was supposed to have allowed before the forceps was to be introduced. But how could any practitioner discriminate these dilatations of two, three, or four-fifths, while a woman was in labour? A less educated finger than Dr. Johnston's would certainly be misled. As to the statistics based on those divisions, he found it exceedingly hard to follow them. They were based on what had no existence in nature—namely, those divisions of the womb into two, three, and four-fifths. The only table he could clearly understand was that in which Dr. Johnston gave statistics as to the number of primiparæ delivered by the application of the forceps before full dilatation of the mouth of the womb. The general impression on his mind was, that the practice had not such extraordinary advantages as were claimed for it; and he thought they should hesitate before they allowed to go forth, on the authority of the Dublin School of Midwifery, that it was a good or safe practice to substitute operative skill and manual dexterity for patience and time in dealing with parturition. In the great majority of the instances in the table to which he had just referred, labour would probably have terminated favourably without any interference of the kind at all. The forceps might be used with success by practitioners as skilful and accomplished as Dr. Johnston, but his skill would not be possessed by all who should read his paper and agree with his practice. Some would be tempted to say to themselves, why

should they spend eight or ten hours by a bedside, when all they had to do was to put up their hand, dilate the mouth of the womb, drag out the child, and go home confident that all would be well. The result would be that the forceps would be used indiscriminately, and discredit would be brought upon its legitimate and proper use. They should be careful how far they allowed manual dexterity to be substituted for the efforts of nature.

DR. KIDD said this paper was the most important he had ever heard read in the Society, tending as it did to revolutionise the practice of midwifery, and it behoved the members to discuss it most carefully and fully, for by the reception it got the character of the Dublin School would be judged all the world over. There was no member of the Society who was not attracted to approve of the practice recommended by Dr. Johnston, by the respect all entertain for him both personally and professionally, as well as by the care, clearness, and candour with which he states the results he has obtained, and the earnestness and zeal with which he advocates his principles; and he was confident nothing would please Dr. Johnston better than a careful criticism of his paper, if done honestly and fairly. On former occasions, when the subject was before the Society, he (Dr. Kidd) said he had expressed very fully his dissent, on general principles, from the mode of practice it was sought to inculcate; he could now add very little in this way to what he had already said, nor did he know of anything he would wish to retract or alter; he would, therefore, on the present occasion direct his remarks, as the author of the paper in the concluding paragraph suggests, to an inquiry whether the statistics brought forward prove that the practice recommended is not alone safe and justifiable, but also is a great preservative of the lives of both mothers and children.

Fortunately we possess in the published reports of the same hospital, from the records of which the present statistics have been drawn, the means of comparing the result of the old and the new practice; but it must be admitted that this comparison is in some respects very liable to error, and full of difficulty. Two great sources of error at once present themselves, and must be guarded against as well as may be. In instituting a comparison of different modes of practice, it is, above all, necessary that the cases compared shall be similar, and it is in this respect the greatest difficulty in the present instance occurs, for while in the earlier reports that part of Denman's definition of tedious and difficult labours, that they shall exceed twenty-four hours in duration, is strictly adhered to, in the later reports, that of Dr. Shekleton's practice, by Drs. Johnston and Sinclair, and in Dr. Johnston's own report now before us, this is not done, and it is, in fact, impossible to get the cases into parallel classes. Another difficulty arises from Dr. Johnston's having, especially in his earliest reports, had it in view rather to illustrate the nature of puerperal

fever than to show the results of his practice. However, notwithstanding these difficulties, many important lessons may be learned from even such a comparison as it is possible to make.

The paper divides itself into two sections. The first deals with the use of the forceps in the entire number of cases delivered in the hospital during Dr. Johnston's mastership, the second with the use of this instrument during the first stage of labour. The total number of forceps cases mentioned in the first section is 752, out of a total of 7,862 deliveries—being at the rate of 9·56 per cent. Of these 752 forceps cases, some were undoubtedly cases of complex labour—the exact number we have not the data before us to determine. And, on the other hand, there were cases which would come rightly under the head of tedious and difficult labour which were delivered by craniotomy (26) and version (7). But it may, perhaps, be allowed to let these complex cases and craniotomy and version cases balance one another, and take the 752 cases as representing those in which assistance was given in consequence of delay or difficulty in the labour actually occurring or anticipated. If we examine the former reports of the same hospital, it appears that Dr. Clarke, out of 10,199 labours, met with only 183 tedious and difficult ones—being at the rate of 1·79 per cent.; Dr. Collins, 210 out of 15,850—being 1·32 per cent.; and Dr. Charles Johnson, 259 out of 6,634—being 3·90 per cent.; and in each of these reports are included the tedious cases delivered unassisted, as well as those delivered with the forceps or vectis and by craniotomy. Now, if in the same hospital, and, as may be assumed, with the same class of patients, under the masterships of three men such as Clarke, Collins, and C. Johnson, the total number of tedious and difficult cases observed was so low—or, taking the average of the whole three put together, 652 cases out of 32,683, or 1·99 per cent.—and under the management of a succeeding master 752 out of 7,862 require the use of the forceps, it follows that either the latter was (to use Dr. Johnston's own words) meddlesome midwifery, or the former was negligent midwifery. To determine this point, the author of the paper refers to his statistics as proving that his practice was the best for both mothers and children. Of the 752 mothers, he says, 58 died, or nearly 1 in 13; whereas in four reliable tables of forceps delivery he finds that out of 531 deliveries there were 70 deaths—average 1 in 9½.

This comparison, however, is open to the objection that the things compared are not parallel. If one man deliver a number of patients with the forceps who, if left alone, would have delivered themselves unaided, and another use the instrument only after the natural efforts have failed in consequence, it may be, of some mechanical obstruction to the passage of the child through the pelvis, it is obvious that the results of the two modes of practice cannot be compared. The mortality in the forceps cases of the latter may be much higher than that of the former, and

yet the rate of mortality out of the entire number of women may be lower, proving the latter to be the better practice. The only real way to decide the relative merits of the two modes of practice is to compare the results obtained among the entire number of patients treated on each plan; and the accurately preserved records of the same hospital afford very fortunately the means of doing this also. At page 6 of the report of Dr. Shekleton's mastership, by Drs. Sinclair and George Johnston, a table is given showing the number of patients delivered in the hospital under each master from its foundation till the expiration of Dr. C. Johnson's mastership, showing the mortality of both mothers and children under each. An abstract of this table will serve as a standard of comparison, and for convenience the percentages are here worked out:—

Date	Masters' Names	Total No. of Women delivered	Women Died	Per cent.	Children Dead-born	Per cent. of Total Born
1757-1759	Dr. Moss - -	915	14	1.53	46	4.9
1760-1766	Sir Fielding Ould	3,800	49	1.28	197	5.11
1767-1778	Dr. Collum - -	4,724	65	1.37	258	5.37
1774-1780	Dr. Jebb - -	5,903	63	1.06	269	4.47
1781-1786	Dr. Rock - -	7,088	54	0.76	411	5.69
1787-1793	Dr. Jos. Clarke -	10,787	124	1.14	580	5.28
1794-1800	Dr. Ivory - -	11,357	86	0.75	600	5.17
1801-1807	Dr. Kelly - -	14,790	163	1.10	974	6.46
1808-1814	Dr. Hopkins -	18,727	217	1.15	1,063	5.57
1815-1821	Dr. Labatt -	21,867	309	1.41	1,535	6.92
1822-1826	Dr. Pentland -	12,885	198	1.53	827	6.33
1827-1833	Dr. Collins - -	16,391	158	0.96	1,017	6.11
1834-1840	Dr. Kennedy -	13,167	224	1.70	651	4.88
1841-1847	Dr. C. Johnson -	13,699	179	1.30	863	6.14
	Total -	156,100	1,903	1.21	9,291	5.85

From this table it appears that, from the opening of the hospital till 1847, when Dr. C. Johnson's mastership terminated, 156,100 women were delivered within its walls, and of these 1,903 died, being a mortality of the entire number of 1.21 per cent., and this may be fairly taken as a correct statement of the results of midwifery of the old school; and under the new mode of practice, out of a total of 7,862 deliveries, 179 deaths

occurred, being a percentage of 2·27, being nearly double the former rate. Disturbing causes may have arisen during the latter period to render this comparison unjust, such as a great preponderance of primiparous patients, or an excessive prevalence of puerperal fever, but of this there is no evidence in the published reports, and certainly, as it stands, it must be taken as proving that the very frequent use of the forceps is not so favourable to the mothers as the more moderate use of that instrument.

The second part of the paper relates to the use of the forceps before the os uteri was fully dilated. The value of that practice must also be judged by results, but, unfortunately, it is difficult to obtain statistics to enable us to do so. He found that Dr. Johnston used the forceps in 169 cases in the first stage, out of 7,862 labours, being a percentage of 2·14. Now Dr. Collins' percentage of tedious and difficult labours was only 1·32. Of the 169 cases, there were 5 of convulsions, 3 of accidental hæmorrhage, 1 of placenta prævia, 3 of disproportion, 6 of deformity, and 1 of laryngitis, making a total of 19 cases to be put out of the calculation. He did not suppose there was a man in the room who would hesitate in any of those cases to apply the forceps if he thought he could deliver the patient before the os was fully dilated. The exclusion of the 19 cases reduced the percentage to 1·90. Out of the whole 169 cases, there were 9 deaths, or 5·3 per cent. Excluding the 5 cases of convulsions, 3 of whom died, it appears that 6, out of 164 delivered with the forceps during the first stage of labour, died, being at the rate of 3·6 per cent. The report does not distinguish the deaths in the cases of hæmorrhage or disproportion. The statistics as to delay in the first stage of labour are, as has been mentioned, very scanty, but Dr. Churchill has recorded 143 cases in which the first stage was prolonged to periods varying from 16 to 176 hours, and in not one of these did the mother die. Moreover, in Drs. Johnston and Sinclair's report of the hospital, under Dr. Shekleton's management, 91 cases of delay in the first stage are recorded, in all of which the mothers were delivered naturally, and without ill effects. Here is a total of 234 cases, of which we have accurate records, and not one death occurred among them, and yet in Dr. Johnston's cases 6 out of 164 died.

Of the children delivered by the forceps in the first stage 21 were stillborn, and excluding 3 that were putrid, the deaths of 18, or 10·6 per cent., may be assumed to have occurred in the birth. In Dr. Churchill's cases 10 were stillborn, one of which was putrid, leaving 9 to be charged to the delay, or 6·2 per cent.

It would appear, therefore, from these facts that it is the better and safer mode of practice for both mothers and children when delay occurs in the first stage, unless there be special circumstances demanding it, to avoid the use of instruments, and instead to rely on milder means for conducting the labour to a safe termination.

DR. M'CLINTOCK said that of the two salient points of practical importance in Dr. Johnston's paper—viz., (1) the employment of the forceps *before* the full dilatation of the os uteri; and (2) the more frequent use of the forceps in all cases of labour, he would confine his observations to the former. He (Dr. M'Clintock) thought that to Dr. Johnston indisputably belonged the credit of having clearly laid down and taught the practice in question, be it good or bad; as he (Dr. M'Clintock) was not aware of any obstetric authority who previously had recommended the use of the instrument under these special circumstances. Dr. Johnston laid it down that in all the cases where the practice was resorted to the os uteri was in a dilatable condition, and, no doubt, this was a very important condition for safety and success. In all the cases reported in Dr. Johnston's paper, chloroform was also freely administered, and this tended powerfully to promote the dilatation of the os. It was a remedy in which he (Dr. M'Clintock) placed great confidence for effecting the same object. But was this operation practicable and safe? Anyone might well imagine, *à priori*, that the great danger to be apprehended was laceration of the os uteri; for, in fifteen or twenty minutes, the forceps accomplished what nature would take several hours to effect. According to Dr. Johnston's tables 44 cases were delivered with the forceps when the os was only two-fifths dilated, and of these 6 died. *Post mortem* examination of these 6 cases showed that in *none* was the os more fissured than it usually is after a first confinement. In primiparæ, as we all know, it was quite common for the os to tear without any evil result. Again, Dr. Johnston employed the forceps 46 times in pluriparæ, where the os was only imperfectly dilated, and not one of these patients died. The mortality was greatest in the cases in which the os was least dilated. In the first group (in which the os was two-fifths dilated) the mortality was 1 in 9, in the next class (of three-fifths dilatation) the mortality was only 1 in 30, and in the class next to that it was 1 in 38. The average mortality in all the cases was only 1 in 18 $\frac{7}{8}$. From these facts he (Dr. M'Clintock) concluded that the use of the forceps was a practicable and safe operation, and that it was, therefore, a justifiable operation in circumstances where any other alternative would seriously compromise the safety of mother or child. The dilating force acting on the os when the forceps was used was from within, and so far followed the course of nature. The cases to which this early resort to the forceps was most likely to be applicable were—1, convulsions; 2, hæmorrhage; 3, pro-lapse of the funis; 4, certain cases of tumours obstructing delivery; and 5, certain cases of pelvic distension. Whilst he (Dr. M'Clintock) thus spoke in favour of the operation, he wished at the same time to express his strong conviction that the forceps should not be so employed except under extreme circumstances; nor should it then be used except by practised hands, as, under these exceptional circumstances, its employment required

more than ordinary caution and skill. It is true the results of Dr. Johnston's cases were most successful, but we must remember that in all his cases the instrument was used by most experienced hands.

The PRESIDENT said that, at so advanced an hour of the evening, his observations must necessarily be very brief; indeed, a great deal of what he had intended to have said had been taken out of his mouth by Dr. M'Clintock and Dr. Kidd. He thoroughly agreed with Dr. M'Clintock that to Dr. Johnston belonged the credit of having originated this practice. His friend and former colleague, Dr. Johnston, always entertained a great regard for the forceps, and this regard has now ripened into an ardent love. Dr. Johnston was also the first to change the practice with regard to the forceps in the Dublin Lying-in Hospital, when he (Dr. Johnston) was senior-assistant to the late Dr. Shekleton, former chief medical officer of that institution—the rule was never to use the instrument in a primiparous case. He, the President, knew Dr. Shekleton was accustomed to say, “I would rather cut off my right hand than put on the forceps in a first case!” One day, however, when Dr. Shekleton was absent, his friend, Dr. Johnston, successfully operated upon a primipara, and proved that the instrument could be used in such cases with perfect safety, as he has now done with respect to its use in cases where the os uteri has but slightly dilated, and which he, the President, believed to be a great acquisition to the practice of obstetric surgery. Dr. Kidd had dealt ably with the statistics illustrating the question under debate. To the President it appeared that the mortality to the mothers in cases of delay during the first stage, and both first and second stages, when left to nature, was about the same, if not absolutely the same, as when the forceps was used in Dr. Johnston's easy forceps applications, so that Dr. Johnston's practice does not increase it. Therefore, the President did not think it was justifiable to use them as frequently as Dr. Johnston had done in such cases, he not having shown that his practice lessened the maternal mortality to any extent whatever. He (the President) quite agreed with Dr. M'Clintock that the dilatation of the os from within was safer, easier, and more rapid than from without (as evidenced in footling and version cases), and the former effect was produced by the forceps in these cases. He, however, was certain that a number of cases in which Dr. Johnston had used the forceps for tardy dilatation of the os uteri would, if left to themselves, have done very well, and that therefore a great number of his operations were unnecessary. In the course, now, of a long and large practice, including 6,000 cases in his maternity, the President never found it necessary to use the forceps where the os uteri was so little dilated as in Dr. Johnston's cases; and he hoped not long hence to give his statistics to the Society, which will demonstrate that his maternal mortality has been extremely slight.

DR. JOHNSTON, in reply, said: Mr. President, as I have only one or two observations to make, which will not occupy more than a few minutes, I do not ask for an adjournment—particularly as Dr. M'Clintock's and your remarks on the different salient points of my paper have, to a great extent, answered most of the objections that have been raised. In the first instance, however, I cannot avoid taking notice of the statement Dr. Atthill has just now made—viz., "That he used the forceps twenty years ago, before the os was fully dilated." Now, as this is the first time I ever heard of his having done so, I cannot help expressing my surprise that he should have allowed such a length of time to elapse before mentioning it, more particularly as he presided on three occasions when I read my Reports—viz., for the years 1872-3-4, and although he each time made his comments on the practice, still he never alluded to his having performed it. As to his observations with regard to "the dragging down or prolapse of the cervix," which, he states, "takes place so frequently during the operation," my reply is, that such a calamity never occurred in my practice, nor have I experienced "any difficulty," nor was there ever any "great force required" (as he has expressed) "in the dilating of the os." On the contrary, it is remarkable how very easily it relaxes. As Dr. M'Clintock has observed, "the dilating force being from the interior, the natural direction, the os yields far more readily than when the expansion is attempted from without."

Then in Dr. Kidd's observations with regard to the comparative mortality in mothers and children being unfavourable to the practice, he says:—"The only real way to decide the relative merits of the two modes of practice is to compare the results obtained among the entire number of patients treated on each plan;" and in order to test the effects of the frequent use of the forceps on the rate of mortality among the mothers, Dr. Kidd has adduced very large statistics from the Dublin Lying-in Hospital records, comparing the death-rate under the Masters who very rarely employed the forceps with those who used it frequently. Finding that the rate of mortality was less under the former than under the latter, he infers that "it must be taken as proving that the very frequent use of the forceps is not so favourable to the mothers as the more moderate use of that instrument."

Now, the conclusion I respectfully, but firmly maintain, is altogether a *non sequitur*. The experience of all lying-in hospitals demonstrably proves that the most influential agent in the production of the mortality among the women is puerperal fever. When it is absent the mortality is small, and *vice versa*; and unless Dr. Kidd is prepared to prove that the use of the forceps strongly favours the development of puerperal fever, his line of argument is fallacious. I maintain, therefore, that the practice I have set forth is a favourable one, as is proved by the following statistics:—

	Mortality of Mothers.	Mortality of Children.
From Drs. Hardy and M'Clintock's Reports -	1 in 6½	No report.
In Dr. C. Johnson's 7 years' hospital practice	1 in 7	1 in 2½
From Dr. Shekleton's Reports, by Drs. Johnston and Sinclair - - - - -	1 in 18	1 in 4½
In Dr. Denham's 7 years' hospital practice -	1 in 6½	1 in 6½
In Dr. G. Johnston's 7 years' hospital practice	1 in 13	1 in 14
Before the os uteri was fully dilated, in 169 cases	1 in 18½	1 in 8*

And let us, when viewing the mortality of the mothers in the 169 cases, delivered before the os was fully dilated, take into consideration the circumstances under which they occurred, viz. :—

3 being cases of convulsions, brought into hospital comatose.

2 being cases of gastritis, from which they were suffering previous to admission.

1 being a case of extreme delicacy, all through pregnancy, and in which, at *post mortem* examination, "the uterus was found well contracted. No metritis."

2 being cases of innupta, with extreme fretting.

1 being a case of sloughing of vagina from too frequent examination.

Now, as not one of these cases could be attributed to the use of the forceps, I therefore consider I am fully warranted in the conclusion with which I have terminated my paper—viz., "That it is amply proved that the practice is not alone safe, but is also a great preservative of the lives of both mothers and children."

ANEURISM OF THE CORONARY ARTERY.

A VERY rare case of aneurism of the left coronary artery, opening into the right ventricle, is reported by Pistoni in *Lo Spallanzani* (fasc. 5, p. 201, 1878). The aneurism formed a tumour, the size of an apple, corresponding to the base of the heart and on its anterior surface. It communicated with the right ventricle by means of an opening situated at the external part of the conus arteriosus. The walls of the ventricle had lost their muscular character, and in contiguity with the tumour appeared to be formed of dense fibrous tissue; there was also endarteritis deformans of the arch of the aorta which had led to the formation of another aneurism on the posterior surface of its ascending portion. The clinical details are not given.—*La Medicina Contemporanea*, Nov. 1878, p. 450.

* The reason for Dr. Collins' Reports not being quoted is that they do not contain any table showing the result of forceps cases.

TRANSACTIONS OF THE ULSTER MEDICAL SOCIETY.

SESSION 1878-79.

President—A. HARKIN, M.D., J.P.

Hon. Secretary—WILLIAM WHITLA, M.D.

First Meeting, 28th November, 1878.

A. HARKIN, M.D., President, in the Chair.

President's Address.

GENTLEMEN,—In accepting, at your invitation, the honourable position of President of the Ulster Medical Society, my first duty is to thank you most warmly for the high compliment you have conferred upon me, and to express the hope that, with the aid of the Council and the indulgence of the Members of the Society, I shall be enabled to preside at its discussions to the general satisfaction.

I have great hopes that a new era has dawned on our time-honoured Society. Last session afforded evidence of renewed energy and life, and discussions on important topics and valuable monographs contributed to maintain an interest in its proceedings. I have learned, too, that in the coming session there is every prospect of progressive improvement.

I trust that the younger members will not hesitate to bring forward original papers and disquisitions on medical subjects through fear of friendly criticism, and that the seniors will endeavour to overcome the *vis inertiae* by which they are too liable to be influenced, and that they will cull from the repertory of their note-books some of their important cases and theoretical speculations. Labouring thus in unison, we may have good reason to hope that when the session terminates the retrospect shall be satisfactory to us all.

My able and accomplished predecessor in this chair treated in an exhaustive manner, in his opening address, the subject of the great advancement of medical science—its conquests and its triumphs in modern times. The task which I have imposed on myself will, I fear, be less flattering and agreeable; for it is my purpose to advance opinions adverse to those generally recognised, and to question the propriety of the teaching and practice on a very important subject—"the Milk-feeding of Infants at Nurse." The proposition which I hope to establish is, that in the unreasonable and excessive dilution of cow's milk practised by mothers and nurses in the feeding of infants, sanctioned and taught by

many members of our profession, serious injury is done to the nursing child, its natural development retarded, its growth stunted, and too often the seeds of disease and death implanted in its constitution.

Were every English mother actuated by a sense of her maternal obligations, did she nourish her baby during the early months of its existence with the food specially provided for it by the Author of its existence, defective alimentation could not, with justice, be classed as a potential cause of infant mortality, nor should it be in my power to animadvert on prevalent errors in artificial or hand feeding of children, the main resource of the fashionable dames of modern days. How strange, and how much to be deplored, that the Christian mothers of the 19th century should be as oblivious to duty in this respect as the matrons of Pagan Rome in the age of Tacitus, who, as a proof of the degeneracy of Rome in his days, laments that while in former times "grave matrons attended to their children as their first duty, they now," he complains, "entrust them to the care of some Grecian slave, or other inferior domestic."

But in addition to the class of unwilling mothers, there is another and a numerous one to whom Nature has not been so liberal, and who have not been provided with the maternal nourishment, and who are, therefore, precluded from performing that duty; and a third class who, although secreting freely, are debarred from nursing by the defective quality of their milk through a tuberculous or other taint in their system.

It is in vain that, according to the apostle, "the babe desires the rational milk without guile," for "every nurse and mother," in the words of Sir James Simpson, "thinks that she can improve upon God's food—pure and unadulterated milk." Some nurses from the very first week are in the habit of adding farinaceous food—such as corn-flour and arrow-root—to the posset of milk, in happy ignorance that it is not until the lapse of five or six months that the infant acquires the property of digesting starchy food, and that, in the absence of ptyalin, it passes into the stomach and bowels in an insoluble state, incapable of being assimilated—intestinal trouble being the necessary result of such feeding.

But the greater number—and it is with those that we have now to do—prefer to rear their children on that food alone which most nearly resembles the mother's milk, and therefore the milk of some inferior animal, when a wet nurse is not provided, is depended upon for their nutrition. The milk of the ass, though suitable for some cases, frequently disagrees, and the milk of the goat is not always available, so that practically cow's milk is the chief reliance of the hand-fed child. Milk as it comes from the cow is a beautiful emulsion—a compound fluid in which sugar, curd, and oil, are mingled with a certain proportion of water—and admirably adapted to the digestive powers of the infant. While the water and sugar are absorbed, the curd is separated by coagulation, and finally

dissolved by the agency of the gastric juice, which is much more powerful in the young than in the adult, and thus greatly contributes to digestion. If then this bland fluid, moderately heated, were given without any of the improvements alluded to, especially the benevolent addition of water in scientific profusion, we should have fewer puny, sickly infants to prescribe for; fewer complaints of colic, of spasms; fewer entreaties for carminatives, for sedatives to still the voice of hunger in the poor starvings whose only requirement is to be found in a cup of genuine unadulterated milk. But the doctor is consulted—he finds that the infant is not thriving, that it is peevish, languid, restless by night and day, that its evacuations are unhealthy, that it is constantly wetting its napkin, that its countenance is pale and anæmic, its forehead and scalp traversed by large blue veins, that diarrhoea is a frequent visitor, and that if a change do not soon occur, the gravedigger shall soon have his due—every fourth or fifth grave that is dug being for an infant under twelve months; but then—

“ ’Tis not a life—

’Tis but a piece of childhood thrown away!”

The doctor soon discovers that there is a great defect in the nutrition of the child, and inquires upon what it is fed. He is gravely answered—“Upon milk and water.” He asks, why give water? Because, the nurse replies, *cow’s milk is too strong for the child*, and we add equal parts of water—or, two parts of water to one of milk!! or frequently three parts to one of milk!!! And upon this watery diet you expect the infant to thrive, and you wonder that the water you are incessantly pouring in by the mouth is as constantly passing by urination, that its flesh is not firm, and that its bowels are so relaxed.

On inquiring of trained nurses for an explanation of this practice, I have been answered, “Oh, Drs. A. and B. always do so;” by one that she was taught the science of dilution, 2 parts water 1 milk, in the Rotunda Hospital. My reply was, “Perhaps so.” I have no doubt its present masters give different advice. By another, that an eminent gynæcologist in Edinburgh told her always to give equal parts. “If so,” I answered, “while I have every respect for that gentleman’s scientific acquirements, it is evident he has yet some knowledge to gain.”

But passing from the statements of monthly nurses and mothers, what do our text-books, what do our teachers say on the subject? I shall quote from but a few, as I am sure the generality of the practice of dilution will not be questioned. Underwood, a great authority in his day, directs a portion of water to be added, leaving the quantity to the discretion of the nurse. Dr. Combe, “On the Management of Children” (p. 234, Chap. XI., on Artificial Nursing), says—“At first two-thirds of pure fresh water should be added to one-third of milk, but goats’ or asses’ milk does not require more than an equal part of water; after a week or two the

quantity of water may be reduced to one-half, and afterwards to one-third, at which proportion it should remain for four or five months." Some text-books tell us to give the milk diluted with a proper amount—some two-thirds water to one-third of milk, some with equal parts. *The Edinburgh Medical Journal*, quoted in *Brit. Medical Journal*, July 15, 1873, publishes the following, among other rules, for the management of infants, prepared by the medical officers of the New Dispensary, and circulated among the visitors to the charity—further assuring us that they are very similar to those which have been issued for some years at the Middlesex Hospital and the Hospital for Sick Children, London. After some very useful paragraphs—Paragraph 3, how to bring up by hand:—"If the child must be brought up by hand, it should be fed with milk and water out of a bottle. At first there should be nearly as much water as milk, but when the child is a month old two parts of milk should be mixed with one of water; after this the proportion of milk should gradually be still further increased, till at five or six months it is given plain." The editor of *The Edinburgh Medical Journal* adds this sensible remark:—"To mix water with milk is needless—too much of that is added before the poor mother gets it, and, even were it not so, the child has need of all the nourishment it can get."

This expression of opinion by the editor formulates in a few terse words my long-cherished idea, the result of my personal observation and experience during the greater part of my professional life, and to enforce its adoption and to induce my professional brethren to reconsider their teaching and practice on this important subject is the aim and object of this paper. I shall trouble you with but one other quotation from a very learned and the latest writer on the Science and Practice of Midwifery, Dr. Playfair. He says, at p. 276, Vol. II.:—"A common mistake is over-dilution, and it is far from rare for nurses to administer one-third cow's milk to two-thirds of water—the necessary dilution will be best obtained by adding to pure, fresh, cow's milk one-third hot water, so as to warm the mixture to about 96°. After the first two or three months the amount of water may be lessened, and pure milk, warmed and sweetened, given instead."

You perceive, gentlemen, dilution is the prevalent idea, with one bright exception, in the hand-rearing of children—it is by this potent means that we shall succeed in putting a bone into children when they are young. But you will not fully realise the grandeur of this idea, the complete development of this gospel of nutrition, unless you take the trouble of following the process step by step.

In the first place, according to Letheby, the normal amount of water in good honest milk ranges from 84 to 95 per cent.—a fine foundation for the exercise of the noble act of dilution; to this the dairyman is permitted, by custom, sanctioned by the Society of Public Analysts, to superadd 5 per cent. from the cow with the iron tail; the dealer next

appears upon the scene, and he is permitted by the authorities of Somerset House to add 15 per cent. on his own account, as they have declared that milk with 20 per cent. of added water should be passed as genuine. The purchasers are thus entirely at the mercy of the dealers, who, too often, by dilution and skimming of the milk, rob it of 25 per cent. of its cream and other nutritive qualities. According to Dr. Cameron, however, while 25 was a usual rate of dilution in Dublin, it frequently rose to 60 per cent. of added water. Let us now follow this sophisticated fluid, this ill-used aliment, to the nursery, and here we shall find that it receives the worst of all bad treatment, for before the passive infant is permitted to take the pellucid fluid it is again doctored by the addition of, not 5 or 10, but of 100, 200, or very frequently, 300 per cent. of water, by the advice or tacit sanction of the physician, and on the plea that the pearly liquid is too strong for the digestive organs of a healthy child! Surely the homœopathic nostrum of the third dilution could not compare in absurdity to this. To what purpose, may I ask, the interference of the Legislature rendering it a penal offence in a dealer to dilute or adulterate the milk beyond a certain proportion—to what advantage the action of the magistracy who daily, in our courts of law, impose heavy penalties on conviction of the adulteration of milk, assigning as a reason that it deprives the citizens of good and proper nourishment, if heads of families, through ignorance or unsound advice, so often nullify the intentions of Parliament, and mix and destroy the most salubrious of infantile foods? But where, I ask, in this deluge of water is the poor infant to find the necessary amount of nitrogenous and carbonaceous material? where the saline principles indispensable to the growth and development of the solid textures of the animal frame? But as those matters, so essential to vitality, are not presented to the infant, as a natural consequence defective alimentation leads to individual degeneracy, and ultimately to progressive degeneracy of the race, to wasting disease, and to death.

“How sad to mark how soon the flower of human life
Hastes to its fading from its very birth!
Another newly-born! How near are they—
The one that's dead, the one that's born to-day!”

As a general principle, it cannot be questioned that disease and premature decay are intimately associated with the nature of the food-supply. According to Ancell, milk, poor or defective in its staminal principles, will, no doubt, produce in the nursling all the effects of deficiency of food, and that debility which operates as a predisposing cause of tuberculous and other diseases (*vide* Ancell, “On Consumption,” p. 453); and Donn  informs us that he ascertained, “by direct experiment, that an inappropriate nourishment of young animals has a great effect in altering the shape and nature of the corpuscles of the blood” (*vide Microscopical Journal*, 1842, p. 245). It is a long time since Mr. Phillips declared that

dilution cannot make cows' milk resemble that of women. Physiology and pathology alike protest against the false principle which I have been arraigning to-day ; but my chief reliance shall be upon chemical research to upset the assumption upon which this hypothesis rests—viz., that the milk of the cow is so much stronger than human milk that it requires an amount of water, varying from 25 to 300 per cent., according to the whim of the blender, to assimilate the fluids, and adapt cows' milk to the digestive capacity of a healthy child.

In accordance with ordinary modes of reasoning, if 300, 200, or even 25 per cent. of water were required to reduce a certain liquid to the strength of another, it should only be so because that liquid was composed of elements three times, twice, or one-fourth stronger than the other. Now, is such the case with the two liquids we are comparing? Plainly, is the milk of the cow three times, twice, or even one-fourth stronger in nutritive elements than human milk? Let chemical analysis determine. In the first place, the specific gravity is almost identical, varying in both, according to conditions of health, from 1013 to 1832 (Vernois and Becquerel). According to analysis of Regnault, given in "Neligan," in 100 parts of human and cows' milk there are found—

	Cows' Milk.	Human Milk.
Water - - - -	87·4	88·6
Oil and butter - -	4·0	2·6
Lactin and soluble salts -	5·0	4·9
Casein, albumen, and fixed salts -	3·6	3·9
Total solids, cows'	12·6	11·4
„ human	11·4	
Difference - -	1·2 (about one-tenth).	

Thus, on the computation that the milk is obtained direct from the cow, about one-tenth is the exact amount of solid nutriment contained in that secretion as contrasted with the milk of the mother ; and upon this slender superstructure the edifice of dilution is established !

But to act in every case upon the principle of dilution is simply to become the slave of routine ; and, in my somewhat extended experience, I have very seldom found it necessary to add any water to the milk provided for healthy infants, for the mere purpose of dilution. I have occasionally advised the addition of an ounce or two of lime-water to a pint of milk, not as a diluent, but as a corrective of lactic acid, which is frequently found in milk when not absolutely fresh—the normal reaction of good milk being alkaline ; and often that corrective is not necessary, for if the milk occasionally disagrees on account of the varying condition

of the digestive organs of the child, or from other causes, it is only necessary to boil the milk and carefully skim it before use.

Children nourished with the fresh milk of the cow present a rosy, robust, happy, and contented appearance, in marked contrast with the poor starvings fed upon homœopathic doses of milk administered in floods of water, or mingled with starchy or farinaceous diet. No one can adequately tell the amount of infantile mortality due to inefficient feeding; thousands whose deaths are annually registered as caused by teething, diarrhoea, water in the head, tabes mesenterica, convulsions, thrush, &c., and who perish in the first year of their existence, in reality owe their deaths either directly or remotely to improper feeding. Should not our sympathies, then, be directed

“Towards the young souls new clothed in helpless clay,
Fragile beginners of a mighty end?”

And if we could better instruct their mothers in this all-important duty, many lives would be saved to the State, infants would not die as if to make way for others to be born, and some reparation would be made by the present race of medical men for the false teachings of other days, the traditions of which still linger and are painfully evident in many customs of modern life. When a fond mother exposes her child to a cold chilling wind, with the avowed object of hardening its constitution, she is but obeying the dictum of Dr. Underwood, who taught the mothers of the preceding generation to do so, and who testified to “the absolute necessity of inuring very young infants to endure the cold as essential to their health;” and that of Dr. Armstrong, another great authority, who taught at the same time that infants should be daily plunged into cold water, even the day after their birth, for the purpose of bracing up their nerves, &c. We are also confronted daily with the senseless dogma, believed in by most mothers, that because a child is teething it would be dangerous to stop a diarrhoea, and many an infantile life I have known to be sacrificed to such a vulgar error.

My great object in bringing this subject before the profession is not, I trust, a too ambitious one, if I express the hope that the medical men of the present day—the legitimate advisers of mothers and nurses—will set their faces against this monstrous and wide-prevailing abuse; and that, while it may be necessary in some cases for a time or in certain conditions of an infant’s health to permit or advise the addition of some diluent to its ordinary pabulum, they will not permit the use of their high authority as sanctioning a process of promiscuous dilution.

THE DOCTOR
SOCIETY FOR
MEDICAL
OBSERVATION

COMMENTARIES ON DISEASES OF THE KIDNEYS.

PART I.

By ARTHUR WYNNE FOOT, M.D., Univ. Dubl.; Senior Physician, Meath Hospital and County of Dublin Infirmary; Fellow and Ex-censor, King and Queen's College of Physicians in Ireland; Diplomat in State Medicine, Trinity College, Dublin; Lecturer on Practice of Medicine in the Ledwich School of Medicine and Surgery; Fellow, Royal Geological Society of Ireland, &c.

[Continued from page 532, Vol. LXVI.]

ALBUMINURIA in inflammatory affections of the kidneys—Its source—Conditions in which largest amount is found—May amount to 5 p. c.—i.e., more than half the amount contained in normal blood serum—Differences between the urine in inflammatory and congestive renal affections. **HÆMATURIA** as a symptom of renal disease more rare than albuminuria—It implies and involves albuminuria—True and false, or extra and intra-renal H. as in case of albuminuria—True Hæmaturia as a result of venous tension (as in cardiac disease), or as part of inflammatory process (as in nephritis)—False Hæmaturia result of admixture of blood from calyces, ureters, bladder, urethra, or female genital tract—Characteristic appearances of urine in true H.—Coagula, form and quantity—Mechanical obstruction from—Lumbricoid thrombi—How to appreciate fully the diagnostic value of coagula in the urine—Characters of urethral and vesical in contrast with renal hæmorrhage—Circumstances which interfere with microscopic examination of blood corpuscles in the sediment—Conditions which favour it—their peculiarities in urinary solutions—diffusion of their colouring matter. False Hæmaturia characterised by absence of red corpuscles—synonyms: Hæmatinuria, Hæmoglobinuria, Intermittent Hæmaturia (Harley), Paroxysmal Hæmaturia (Pavy), Intermittent Hæmatinuria (Gull). Description of the complaint—Vicarious Hæmaturia. *Causes of Hæmaturia.*—Injuries—Cancer—Active and Passive Congestion—Inflammation—Calculi—Corpuscular necrosis. **THE FORMED ELEMENTS OF THE URINE IN RENAL DISEASE:**—(a.) Normal—Uric acid, calcium oxalate, earthy phosphates. (b.) Abnormal—Blood corpuscles; epithelium (renal); "casts," etc. *Renal Epithelium*, its important properties and functions—Bowman's view of the renal secretion—Ludwig's conception of the same—Heidenhain's experiments, showing the secreting power of the epithelium of the convoluted tubules—Possible conversion of kreatin into urea by the renal epithelium—Renal epithelium as a sedimentary deposit. **RENAL CASTS.**—Varieties of: (a) Epithelial casts; (b) Blood do.; (c) Hyaline; (d) Granular; (e) Waxy. Discovered by Henle (1842). Proofs of the seat of origin of casts—Objection to the common term of "fibrinous" casts. Investigations of Axel Key on their structure—usually formed in the looped tubes—their onward movements. Relation between tube casts and albumen—Significance of tube casts—Value of the character of the casts—Indications to be derived from different varieties of casts. **CONSTITUTIONAL DISTURBANCES IN KIDNEY DISEASE:**—(a) *Dropsy*. (b) *Uræmia*. (c) *Dyspepsia*. Cause of the dropsy—Hippocrates acquainted with renal dropsy—S. G. of the

blood serum as low as that of the urine (1013)—Hydræmia—Loss of albumen not the sole or even essential cause of the dropsy—Nature of the fluid effused—its law of distribution, fugitiveness, obedience to gravitation, transparency.

THE source of the albuminuria in the inflammatory affections of the kidneys is the capillary coils of the glomeruli, which conduct themselves, in inflammations of the kidneys, just as do the blood-vessels in other organs which admit of being examined under the microscope while they are inflamed. Compared with all other causes of albuminuria, inflammation of the kidneys—and especially that form of it which runs a chronic course—furnishes the largest percentage amount of albumen to the urine. In such cases the quantity of albumen can reach 5 per cent., which is more than half the amount contained in normal blood serum. In inflammation of the kidneys an exudation of blood plasma and white blood-cells takes place through the walls of the vessels, and becomes mingled with the urine secreted by them, and on this account the character of the urine furnished, when the renal vessels are thus abnormally affected, serves to distinguish it in a marked manner from that secreted by kidneys which are merely in a condition of congestive hyperæmia. Thus, in the urine furnished by inflamed kidneys we discover albumen and fibrin—the elements of the blood plasma, and find white blood cells, as a rule, in the sediment—the latter appearing sometimes in immense numbers, and being mixed with large quantities of red blood cells in the acute cases of nephritis of decided severity.

We now come to the consideration of the passage into the urine of the red blood corpuscles, or of their colouring matter. This event, which is termed *Hæmaturia*, may be accepted as a symptom of kidney disease, although it certainly occurs more rarely as a symptom than does the escape of serum albumen alone. Bloody urine is invariably albuminous—for when the red corpuscles pass into the urine a notable quantity of albumen accompanies them. The red corpuscles may escape from the vessels of the kidneys into the urine either as a result of mere venous tension, as is observed in the hæmaturia of cardiac disease, or as an accompaniment of the inflammatory process, as is seen in nephritis; or they may become mixed with urine which was secreted in a normal state, but into which blood afterwards escaped either from the calyces, the ureters, the bladder, the urethra, or the female genital tract.

The blood shed into the urine at its very source in the kidney betrays itself by a characteristic colour that ranges between a smokiness like beef-tea and the blackness as of porter, according to its quantity; intermediate degrees are often compared by patients to the colour of strong tea or bog-water. In every instance the blood-cells will be more or less burst and robbed of their true colour, if the bleeding have taken place in the kidney. The fibrinous constituents of the blood plasma, after they have escaped from the walls of the vessels into the urine, in hæmaturia,

coagulate into firm clots which entangle the renal epithelial cells or other formed elements with which they may lie in contact at the moment of coagulation. The form and quantity of these coagula excreted with bloody urine vary with the seat of the bleeding and the amount of the blood. Thus, the more copious the bleeding the more numerous they will be; and if the hæmorrhage takes place in a roomy part of the urinary apparatus, the larger will be the size of the clots. Thus, in the bladder, huge blood-clots may be formed which cannot pass out through the urethra without suffering diminution of size induced by contraction of the bladder, or by mechanical breaking up with instruments passed into the bladder. Firm, rounded clots, corresponding exactly in length and diameter with that of the distended ureter in which they were formed, may occur when pure blood is poured out into these connective channels in cancer of the kidney or of its pelvis; and the passage of these ureter-shaped thrombi, measuring often more than a finger's length, and being of about the size and shape of a lumbricoid worm, has given rise to the assumption of the presence of entozoa (the strongylus, for example), whose existence at all in man has been disputed. There is an account in the "*Trans. Pathol. Society of London*" of a blood cast from the ureter, six inches long, which was mistaken for a worm.^a In hæmorrhages from the pelvis of the kidney blood-clots have been seen in the urine which exactly took the shape and size of the calyces. Such profuse kidney bleeding as leads to the formation of large clots in the pelves of these organs may be observed sometimes in cases of traumatic lesion of the kidney—such as crushing, rupture, or laceration of the kidney, from kicks or other modes of violence—or where a highly vascular cancer has grown into the pelvis of the kidney. If the urine be mixed with blood derived from the substance of the kidney, the quantity of this is, as a rule, so small that the clotting of the fibrin takes place within the renal tubes and before the pelves are reached. The coagula then indicate, by their form and shape, their point of origin, making casts of the channels whence they issued; and though often so small as to escape the naked eye, they may be discovered in great numbers in the urinary sediment when put under the microscope. In order to appreciate fully the diagnostic value of clots of blood in hæmaturia, it is well to bear in mind a suggestion of the late Mr. Hilton,^b which is not to throw away the clots after a cursory inspection, as is usually done, but to swim them out in a white dish or plate, giving them time to unravel themselves, as the shapes they then assume are well and easily seen, and are of great help in the diagnosis of the seat of hæmorrhage, for they will most likely exhibit the well-known form of the space that contained them during the coagulation.

^a Vol. XI., p. 164.

^b Guy's Hosp. Rep. 1867, p. 19.

To glance for a moment in passing at some of the hæmorrhages which, while producing hæmaturia, are yet strictly *extra renal*, it will be sufficient to observe that in females the urine is generally bloody during the menstrual flow, and that it may also become so at any time if there be uterine or vaginal hæmorrhage. Urethral hæmorrhage is indicated by the escape of blood in the intervals between micturition, and by its appearing when the canal is pressed with the finger in the perinæum from behind forwards. Or if you desire the person to make water in your presence, and you see the blood escape before the urine, its origin must be in front of the bladder; and if catheterism is employed, you find the urine drawn direct from the bladder is clear. In vesical hæmorrhage there is usually great pain referred to the bladder when this cavity contains a large coagulum. The blood is recognised by the bladder as an extraneous body, and the muscular walls become spasmodically contracted in order to expel it. The blood also, in coagulation, rests on the surface of the bladder, and the clot itself is somewhat round and flattened in shape, with its edges bevelled off and serrated.

Bloody urine from the *kidney* is more often sooty or dark-coloured than bright red, and the liquid is more usually cloudy than clear. Very small quantities of blood do not alter the colour of the urine much, but even in such cases the presence of blood can often be recognised by the look of the sediment, which presents a brown-black or chocolate-coloured granular looking stratum, or a thin red line-like layer on top of the ordinary deposit, if the urine be allowed to stand at rest in a funnel-shaped glass, terminating at the bottom in a point into which the blood corpuscles can subside. The surest and most trustworthy way, however, of recognising blood corpuscles in urine is by employing the microscope and distinguishing the blood corpuscles in the sediment; nevertheless the corpuscles may disappear very speedily if the urine be of very low specific gravity, or ammoniacal. In acid urine of moderate density (1020–1025) the corpuscles remain visible and preserve their form for several days. The blood corpuscles do not run into rouleaux in the urine as they do when drawn directly from the blood-vessels, but remain discrete and separate. In dilute urine the corpuscles expand somewhat from imbibition; if the urine be more concentrated they preserve more nearly their normal biconcave contour—sometimes they shrink, and become stellate, crenate, or serrate in outline. The diffusion of the colouring matter of the blood through the urine, when the grosser sediment has settled down and the supernatant fluid has become quite clear, may be recognised by a very unequivocal feature, the fluid is dichroistic—i.e., it looks either red or green, according as it is regarded by transmitted or reflected light. If the urine contain very little blood this phenomenon cannot be noticed.

The causes of hæmaturia, apart from the effects of injuries and cancerous

growths, are all those affections of the kidneys which also entail albuminuria—viz., over-repletion of the renal vessels and inflammations of the kidney-substance. Copious and persistent renal hæmorrhages are, perhaps, most common in acute diffuse nephritis, and then the blood escapes into the renal tubules from the capillaries of the glomeruli, altered as these are by inflammation. The presence of calculi in the pelvis of the kidney is one of the most common sources of such an event. Rayer speaks of a patient with renal calculi who had hæmaturia whenever he was compelled to ride. Active congestion, or active hyperæmia of the kidneys, like that produced by irritant substances, such as oil of mustard, oil of turpentine, or cantharides, is also a cause. In persons who have either had large blisters of cantharides applied to them or who have kept some ointment containing Spanish fly as a dressing on their skins, with the object of keeping up an issue, the urine sometimes contains, as well as blood, such a quantity of fibrin that partly even inside the bladder, and quickly after it is passed into the chamber-pot, it forms a firm gelatinous substance; and in such a case the clots formed in the bladder may make the voidance of the urine impossible without the help of a catheter. Mere *venous* congestion, or, as it is called, passive hyperæmia, can reach such a grade that the blood is squeezed *per diapedesin* through the walls of the capillaries into the renal tubules. This is the case, for example, when the obstruction to the outflow of the blood is situated either in the renal vein itself or in the vena cava ascendens, as when a thrombus forms in either of these vessels while the arterial blood-supply to the kidney remains uninterrupted. The venous congestion that follows upon cardiac insufficiency but rarely leads to a distinct mixture of blood and urine; but, on the other hand, hæmaturia occurs in heart disease, and, by no means rarely, as a symptom indicative of hæmorrhagic infarction of the kidney, the result of embolism; but the hæmaturia then is always transient and short in its duration. The hæmaturia which occurs in scorbutus, purpura hæmorrhagica (the morbus maculosus of Werlhof), and in hæmorrhagic small-pox, is not considered by the most recent authorities to be a true renal hæmorrhage, because in most cases of this kind the source of the bleeding has generally been found to be one or the other kidney pelvis, or the blood was derived more rarely from both and from the bladder as well.

There are occasionally cases met with in which, although the urine looks deeply blood-stained, we find no red cells at all to explain the colour, and only a few uncoloured corpuscles under the microscope. Now, in such cases either the blood-pigment hæmoglobin has escaped—the red cells having suffered destruction while still within the capillaries, and the colouring matter having escaped together with the other constituents of the urine—or the red cells primarily excreted with the urine have subsequently broken up in this and have yielded their colouring

matter to it either unchanged or after it has been converted into hæmatin. As causes of this dissolution or destruction of the red corpuscles, may be mentioned the graver forms of fever and the action of particular poisons. Thus, it has been observed in abdominal typhus (enteric fever), septicæmia, and poisoning by phosphorus, by arsenic and its compounds, and by sulphuric acid. In urines thus blood-stained the peculiarity of dichroism just alluded to may be often observed. The surest test for blood-colouring matter dissolved in urine is that by means of the spectroscope. You will remember that the distinguishing feature of the red corpuscles is the presence of hæmoglobin, and that this substance constitutes 90 per cent. of the dried red corpuscles. Although a crystalline body, hæmoglobin is indiffusible, because it is in part a proteid body, consisting of a colourless proteid associated with a coloured compound named hæmatin;^a hence, in normal urines the spectroscope gives no indications of blood-colouring matter. This colouring matter of the blood (hæmoglobin) occupies an unapproachable position among organic substances in point of complexity of constitution, and has been assigned a chemical formula, appropriately termed by Professor Purser^b awe-inspiring, of $C_{600}H_{960}N_{154}FeS_3O_{179}$. It is a substance which very readily undergoes decomposition, the products of which are both numerous and complicated. One of the products of the decomposition of hæmoglobin is a proteid belonging to the globulin family, and it may be to this substance Gerhardt refers under the name of *latent urine albumen*, when he observes that an albuminous substance, precipitable neither by boiling nor by nitric acid, but easily detectable by alcohol, may be present in the urine. The colouring matter, hæmatin, is one of the results of the splitting up of hæmoglobin; and when an acid is added to a solution of hæmatin, the colour of the mixture changes from red to brown, which is probably the explanation of the smoky and tawny tints of various blood-stained urines. Alkaline solutions of hæmatin are dichroic, appearing green in thin, and red in thicker layers. Specimens of urine containing hæmatin, examined with the spectroscope, exhibit an absorption band in the red, the position of which is different in acid and alkaline solutions.

There is a curious form of false hæmaturia, in which attacks of hæmorrhagic urine come on at short intervals, as many as five having occurred in ten weeks. The attacks come on after exposure to cold or damp, and entirely subside in a few hours. During their continuance the urine passed is blood-coloured and highly albuminous, but in the intervals it is normal, and the patients enjoy good general health. Dr. Greenhow has related at length four such cases in the first vol. "Trans. Clin. Soc. Lond." In such cases the escape of the colouring matter of the blood into the urine is unaccompanied by rupture of the capillaries and

^a Foster. Text-book of Physiology. 1878, p. 278.

^b Lectures on the Blood Corpuscles. Irish Hospital Gazette, Dec., 1873, p. 336.

the presence of blood corpuscles. The urine under these circumstances assumes a deep red, burgundy, or blackish-red colour—very much as if it contained blood—but no blood-discs can be found under the microscope, nor any fibrin. Hence it has been called *false hæmaturia*. This condition is invariably accompanied by the presence of albumen in the urine. This form of bloody urine sometimes occurs in a most unexpected manner, and the subject of it is singularly liable to relapses of the affection. Dr. Roberts,^a of Manchester, has drawn up an excellent account of it from an analysis of twenty undoubted examples of the complaint, and from his account I extract most of the following particulars about it. The disease was first noticed by Dr. George Harley^b in 1865, under the name of intermittent hæmaturia. Soon afterwards it received from Dr. Pavy^c the name of paroxysmal hæmaturia. Later on (in 1866) it was called by Sir W. Gull^d intermittent hæmatinuria, from the myriads of hæmatin crystals which he observed in the sediments. The last name proposed for it is hæmoglobinuria, as the urine has been found to show the characteristic bands of hæmoglobin in the spectrum. Certain animals, chiefly oxen, which have been fed in peculiar pastures, are liable to bloody urine—“red water.” This is stated by some to be a hæmoglobinuria, no corpuscles being detected under the microscope, and the urine showing the absorption-bands of hæmoglobin in the spectrum. In the human subject it seems to be almost exclusively confined to males, only 1 out of the 20 cases having occurred in a female. The prognosis is generally good. Of the 20 cases none died, 12 were reported as having completely recovered, 1 was convalescent, and 7 were still in progress when reported. One of the most striking points in the clinical history of this affection is its intermittent character—the urine suddenly, and within an hour or two, changing from a deep blood-colour to a pale straw-tint. The state of the general health seems to vary from robust health to various degrees of delicacy. A rheumatic tendency is a frequent concomitant of the affection; 4 of the 20 cases had at one time or another suffered from undoubted ague; 2 of the cases were actually suffering from ague at the time the hæmatinuria was first observed. In all the other cases (with one exception) the exciting cause was clearly vicissitudes of temperature or exposure to wet. Each paroxysm begins suddenly with a feeling of cold or shivering resembling the cold fit of ague, and terminates with the discharge of a very dark bloody-looking urine. The paroxysms are not followed by any hot or sweating stage. The symptoms then subside, and the urine at the next micturition, or the one after, is found to have resumed its natural, healthy appearance. The return of the paroxysms

^a On Urinary and Renal Diseases. Second edition, p. 139.

^b Medico-Chirurgical Transactions. 1865. Vol. XLVIII., p. 161.

^c Lancet, 1866. Vol. XI., p. 33.

^d Guy's Hospital Reports. 1866, p. 381.

in different cases is most irregular, recurring once a day, or even twice or thrice a day, the patient being apparently quite well in the intervals. Each paroxysm lasts from three to twelve hours. After the paroxysm has lasted for a period varying from thirty minutes to two hours, the patient passes a quantity of dark-coloured urine, having the appearance of porter or of the darkest port wine. The urine is always highly albuminous; it is generally turbid, and deposits, on standing, an abundant, chocolate-coloured sediment, which consists chiefly of amorphous, granular matter, which is, presumably, disintegrated blood corpuscles; and crystals of oxalate of lime are generally seen; casts of tubes are also present—mostly of dark, granular appearance. Curious examples are to be met with occasionally of supplementary or *vicarious hæmaturia*, in which the hæmorrhage appears to take the place of some habitual bleeding—as that from piles—or to be related to some functional disease, as spasmodic asthma. Chopart and P. Frank relate cases in which the catamenia were deviated to the urinary passages, and appeared under the form of a periodical hæmaturia. Chopart and Rayer are authorities for three instances in which hæmaturia occurred at regular monthly periods in males. These were, perhaps, instances of intermittent or paroxysmal hæmaturia. One of them was a butcher of Sedan, and when the circumstance became known, no one would purchase meat from him.

An acquaintance with *the formed elements of the urine in renal disease* is obtained by the microscopic examination of sedimentary deposits. A variety of substances, differing widely in clinical import, are included in the category of formed elements. First, there are those which are at all times contained in normal urine, but which, from their excessive quantity or abnormal form, are calculated to induce disease of the urinary apparatus—as, for instance, uric acid, or calcium oxalate, or the crystalline or amorphous deposits of earthy phosphates, or the rare, and to healthy urine alien, crystals of cystine—all of which may lay the foundation of concretionary formations. Secondly, we meet with elements whose mere appearance in the urine can only be accepted as certain evidence of abnormality, indicating either disturbance of the kidneys' secretory function or actual disease of the urinary passages—such as blood-corpuscles and epithelium derived from various parts of both secreting and conducting apparatus. And, lastly, we may find what are known as “casts,” and recognised as pathological formations derived from the renal tubules. Of all these sedimentary matters *renal epithelium* and the *tube casts* are the most important in the diagnosis of kidney disease.

We have not yet dwelt sufficiently upon the important part played by the renal epithelium in the formation of the urinary excretion. We have alluded to what may be called the filtering part of the kidney—the Malpighian bodies—and have referred to the effects of increased or

diminished arterial pressure upon the secretion of urine. The renal epithelium is held to be endowed with powers of active secretion which make its integrity of the utmost necessity to a kidney in working order. The protoplasmic cells which line at least a large portion of the *tubuli uriniferi*, elaborate from the blood certain ingredients of the urine, and discharge them into the channels of the tubules. Such of the various urinary constituents as are known to be present in the blood—such as sodium chloride—independently of any activity of the kidney, are fairly supposed to be simply filtered through the renal glomeruli; others of them—such as urea—are held to be, to some extent at all events, the products of the activity of the renal epithelium. This is the original view of the renal secretion, as entertained by Bowman—that the urine consisted essentially of two parts: (a) the water and the more general constituents which made their way through the glomeruli, and (b) the distinct specific constituents which were discharged by the activity of the renal epithelium into the channels of the tubules. This older view of Bowman gave way very largely to the conception of Ludwig and his school, who taught that all the urine in a diluted form passed through the glomeruli, and in its descent along the tubules underwent nothing more than an appropriate concentration.. Ludwig's theory regarding the secretion of the urine is this, in fact—namely, that the serum of the blood, less the fats and albuminates, filters through the walls of the glomeruli into the capsules of the Malpighian bodies; the filtrate then passes along the convoluted tubules, and comes into contact with the epithelium and the lymph contained in the lymphatic spaces surrounding the tubules; part of the water of this dilute filtrate is then absorbed by a process of diffusion between the fluid in the *tubuli uriniferi* and that contained in the lymphatic spaces and veins surrounding the tubes on all sides. In opposition to this view of Ludwig, Heidenhain has shown that, with regard to one substance at least, the renal epithelium does exercise a distinct secreting activity, independent of, and separate from, the relations of blood-pressure. He found that sodium sulphindigotate, or indigo-carmin, injected into animals' veins, passed from the blood into the renal epithelium, and from thence into the channels of the tubules, where it was precipitated in a solid form. There were no traces whatever of the pigment having passed by the glomeruli, and the cells which could be seen to take up and eject it were those lining such portions of the tubules as, from their microscopic features, have been supposed to be the actively secreting portions of the tubules. There are reasons for believing that the secreting activity of the renal epithelium is not confined to picking out or selecting the already-formed urea from the renal blood, but that it is also by its agency that the kreatin circulating in the blood is converted into urea, so that it is possible that the kidney may, besides the simpler duty of withdrawing ready-formed urea from the blood, be exercised in transforming certain

nitrogenous crystalline bodies—such as kreatin—to serve as part of the supply of urea which passes from it.*

The epithelium which lines the uriniferous tubes is arranged in a single layer—a distinct cell wall can only occasionally be seen. The nucleus of renal epithelium is roundish, with a delicate, regular outline, resembling closely, both in size and aspect (except in not being biconcave), a red corpuscle of the blood. The nuclei are embedded in a faintly granular protoplasm. As the epithelial lining of the uriniferous tubes is only loosely attached to the basement membrane it is liable to be separated from it in certain diseased conditions and to be discharged with the urine. Renal epithelium forming a urinary deposit occurs usually in scattered patches, but in the acute form of Bright's disease the epithelium is detached in coherent pieces, which constitute casts of the entire lumen of the tubes. The cells macerated in urine hardly ever preserve their normal form, such as may be obtained from scrapings of the cut surface of a *fresh* healthy kidney. The most common change is a greater or less disintegration or breaking up of the cells into amorphous, granular matter, which readily takes place from the absence or extreme tenuity of the cell wall. The cells also become atrophied, and not infrequently are subject to fatty degeneration, which may be significant of changes in the kidney of a most serious nature.

Of *renal casts* there are several varieties, named, some after their structure, others from their most obvious characteristic. Thus, there are *epithelial casts*, *blood casts*, *hyaline*, *granular*, and *waxy casts*. Henle (1842) was the first person who, after he had found casts in the urine of a person afflicted with dropsy and albuminuria, discovered the same objects in the tubules of the diseased kidney derived from the same individual. *Epithelial casts* consist of a cylinder of coagulable matter, studded over with epithelial cells which adhere thereto and are partly embedded therein. Sometimes these casts seem to consist entirely of compacted epithelial cells, or of epithelial cells held together by fibrin so small in amount as to be barely perceptible. The coagulable material which forms the basis or stroma of a cast is separated from the blood in the renal tubes which act as moulds to the nascent material and impress it with their size and shape. Whatever matters the tubes may contain will become embedded in the stroma as it coagulates. It is in the course of acute inflammation of the kidneys that the epithelial lining of the renal tubes may be shed or desquamated in their natural continuity, and appear in the urine as epithelial casts. The *blood casts* are derived from the renal tubules in hæmaturia, and are then excreted in the urine; they consist of coagulable material, and entangle, for the most part, such a quantity of red corpuscles in them as to appear under the microscope perfectly dark and opaque, although the separate blood corpuscles may still be distinctly

* Foster. Text-book of Physiology. 1878. Chaps. IV. and V.

made out. Blood casts show that enough congestion has existed to rupture, or, at least, promote diapedesis through the renal capillaries. They also indicate a disease of recent origin. At least three other kinds of renal casts are frequently recognised in the microscopic examination of a urinary sediment—the hyaline, the granular, and the waxy casts. The *hyaline casts* appear to be perfectly homogeneous, transparent as glass, and so colourless that the outlines are only with difficulty rendered apparent in the surrounding fluid. Their discovery is facilitated by adding to the preparation a solution of iodine in iodide of potassium, or a weak carmine solution, by which means they are stained yellow or red respectively. The *granular casts* have a dark, coarsely-granular appearance; they appear to consist entirely of granular masses, and are, therefore, less transparent to light. The granular matter appears to be of two kinds—one sort produced by the breaking up of detached epithelial cells, the other sort belonging to granular degeneration, and apparently consisting of comminuted and altered fibrin. The *waxy casts* exhibit under the microscope a peculiar, glistening aspect, and often have a distinct yellow staining. They are distinguished from the hyaline casts, which are equally homogeneous, by their highly refractive properties and by their slight coloration. Many, however, do not make any great distinction between hyaline and waxy casts.

If the question is asked of what do these urinary casts consist, it is one not easily answered. That they are formed in the renal tubes there can be no doubt—both because their dimensions agree so exactly with the known dimensions of the uriniferous tubes, and because they have been demonstrated *in situ* in the kidney. They have been commonly spoken of as fibrinous (casts), since Henle, when he first discovered them in the urine, and after he had traced them back to the kidneys, took them for fibrin, and considered that they were derived from blood plasma excreted into the tubules, and which had become coagulated and moulded within them. Now, however, the term fibrinous is falling into disuse, since the learned Swede, Axel Key, has pointed out how completely every kind of renal cast differs from fibrin in its chemical reactions. The very recent investigations of Rovida, of Milan, as far as the colourless and yellow casts are concerned, have led to the conclusion that these cannot consist either of fibrin or of any protein substance—nor can they be formed of gelatin, chondrin, mucin, or hyalin. However, from their possession of certain of the characteristic properties of protein substances, Rovida assumes that they are derivatives of albumen or albuminoid substances. Hence, we must be provisionally contented with the statement that a fluid substance of albuminous nature is poured into the uriniferous tubes where it forms a cylindrical cast by its coagulation. Rindfleisch* is in favour of the view that the epithelial

* Pathol. Histol. (Syd. Soc.) Vol. II., p. 143.

cells, with which the straight tubes are lined, generate a colloid material in their protoplasm which they then pour out into the renal tubes. Wagner^a speaks of the so-called urinary casts having their origin partly in croupous, partly in colloid, metamorphosis of the renal epithelium. The majority of these casts or cylinders are formed in the looped tubes, since the dimensions of those which are met with in urinary deposits correspond more commonly to the calibre of the looped tubes than to that of the wider efferent drains. The onward movement or the expulsion of the cast is effected by the *vis à tergo* of the urine, and meets with no hindrance owing to the extremely flexible and slippery character of the casts, notwithstanding the manifold turnings and windings of the path along which some of them, at least, have to travel. In particular, the passage of the casts from the ascending limb of the looped tubes into the efferent drains takes place very readily, owing to the greater calibre of the latter. Before that doubts as to the fibrinous nature of the casts were raised, it was generally accepted that these casts originated after the same fashion as the casts of the bronchioles in croupous pneumonia, and the view of a croupous nephritis, with a fibrinous exudation into the tubes, was extensively favoured; and in support of this view we have the highly plausible suggestion of Traube, that the rise of the blood pressure in the kidney must needs lead to the filtration of progressively denser blood-constituents through the walls of the vessels—first, of albumen, then of fibrin, lastly of corpuscles. The modern opponents of the opinion, that all forms of urinary casts originate in blood-fibrin excreted with the urine, explain the formation of some of them as originating in a degeneration of the renal epithelium, and in a smelting down of these cells into a homogeneous mass, and that of others as arising from a secretion by the epithelium. The comparative immunity with which they may be present in the urine for long periods of time is accounted for by the lively powers of self-regeneration which Axel Key claims for the epithelial lining of the tubes.

Whatever be the cause of their formation, the elimination of true casts never takes place under perfectly normal conditions, and is, as a general rule, associated with the excretion of albuminous urine. In a large proportion of cases, although not in all, the percentage amount of albumen and the quantity of casts contained in the sediment have a direct relation to each other. For example, in diffuse nephritis the large quantity of albumen implies, without further inquiry, that a great number of casts will be present; whereas the watery urines of the genuine contracting kidney and of amyloid disease—where but a sparing amount of albumen is present—usually furnish but a few casts. The significance of casts in the urine is chiefly this, that they furnish evidence that the

^a Manual of General Pathology. 1876, p. 335.

person who passes them is suffering from true renal albuminuria; at the same time the value of the characters of casts, in assisting to make a diagnosis, is unquestionably great. A urine which contains a great number of pale, or of dark granular casts, comes from an inflamed kidney. If a great number of undamaged epithelial cells from the tubuli uriniferi cleave to these pale casts, and there is a fair abundance of red or white blood corpuscles, and few or no dark granular casts are present in the sediment, the case is one of acute nephritis. If, on the other hand, dark granular casts prevail over the pale and hyaline ones, the case is then one of chronic nephritis. The urine of febrile and congestive albuminuria—as well as that of contracting kidneys, and of most examples of amyloid degeneration—presents only very few casts. The waxy, refracting, and oftentimes yellow-coloured casts, are not confined, as has been maintained, to amyloid degeneration. They indicate a chronic and deep-seated renal affection, and are not present in recent cases of nephritis, nor in transitory albuminuria. Again, the dark granular casts do not appear in the temporary functional disorders of the kidneys; they indicate a notable impairment of the nutrition of the organs; they are found both in nephritis and in amyloid disease of the kidneys.

We now come to a consideration of the disorders which the constitution manifests when the kidneys cease to perform their functions properly, and we at once find ourselves face to face with three great subjects—the *dropsy*, the *uræmia*, the *dyspepsia*, of renal disease. The dropsy which accompanies disease of the kidneys is a result of an insufficient removal of water from the blood; it is due immediately to hydræmia—an abnormal increase of the watery content of the blood-serum. Hippocrates was aware that the diminution of the urinary secretion was a cause of dropsy, and established the source of particular forms of dropsy in the loins (kidneys). It was Bright's work which first clearly established the connexion, as cause and effect, between kidney disease and dropsy. The hydræmia is shown by the very low specific gravity of the blood-serum. Bostock found that the blood-serum of one of Bright's cases presented a specific gravity as low as that exhibited by the urine itself—namely, 1013. The patient from whom both the urine and the serum were derived was dropsical, and passed exceedingly little urine. The coagulum produced by heating the blood-serum was hardly so great as that obtained from the urine when this was boiled. There are many physicians and medical writers who attribute the dropsy of renal disease entirely to the waste and loss of albumen in the albuminous urine—an opinion which is not at once to be rejected; but this loss of albumen is in most cases neither the sole nor the essential cause of the hydræmia and dropsy, and in a good many instances it may be left out of consideration altogether. In evidence of this being the case is the fact that renal disease can lead

to most extensive dropsy, when, throughout its course, no quantity of albumen worth mention has ever escaped in the urine. For instance, dropsy may ensue within a very few days in the acute nephritis accompanying scarlatina and diphtheria—the urinary secretion coming to an absolute standstill, and complete suppression of urine being established—cases which go to show that the non-removal of water from the blood by the kidneys, while the absorption of water from the digestive canal still goes on as in health, can be the source of the hydræmia and dropsy. Those forms of kidney disease in which an abnormal diminution of the urinary secretion take place alone give rise to dropsy with any certainty. In most cases, it is true, the diminution of the renal secretion is contemporaneous with the appearance of albumen in the urine; and it is certainly correct that an abnormal loss of albumen of the blood by other channels—as by a hæmorrhage—may be followed by dropsy. But this in nowise diminishes the importance of the fact that the advent of dropsy in renal disease is directly dependent on lessened secretion of urine, and not upon the quantity of albumen excreted in that urine; for a considerable loss of albumen may take place in this way without any dropsy occurring, if only the quantity of urine passed daily is sufficiently copious, and dropsy may ensue during an insignificant loss of albumen, directly the daily urinary secretion falls below a certain minimal quantity. The dropsy produced by renal disease may disappear, notwithstanding the fact that the loss of albumen remains each day the same, if only the quantity of water secreted by the kidneys becomes greater. The cases of dropsy occurring in particular forms of heart disease favour this view, as in certain cases of heart disease it is only when, in consequence of insufficient cardiac energy, the arterial blood-pressure falls so low that the renal secretion is palpably diminished, that dropsical swellings commence, and then subside again directly the urine-flow is accelerated by the heart's action becoming once more vigorous.

In far the larger proportion of cases of renal dropsy it is not a destruction of those organs which are specially destined to secrete the urinary water—the Malpighian bodies—or the resulting diminution of their number, which leads to lessening of the secretion and so to dropsy, but the obstructions which are opposed to the free discharge of the secretion through the renal tubules. Quite one-half of the urine-preparing apparatus can be wanting, without the other half being insufficient to accomplish its physiological task, as numberless cases of congenital deficiency, and of the entire destruction of one kidney, which are on record, testify. It is not the contracting forms of kidney, associated with destruction of numberless Malpighian bodies, which we find preferentially producing dropsy. It is not uncommon at an autopsy to find the kidneys wasted away to mere degenerate relics, in persons who, during life, exhibited no trace of dropsical anasarca. On the other hand,

dropsy is scarcely or ever absent in the acute and chronic processes of inflammation of the kidneys, in which these organs, through swelling of the epithelium and of the interstitial tissues, have increased considerably in size; for this swelling presents a considerable obstruction to the normal secretion-pressure within the Malpighian bodies, and in this way obstructs the excretion—a circumstance only possible while there is considerable difference between the blood-pressure within the glomeruli and that afforded by the fluid contents of the tubuli uriniferi. It follows that dropsy must ensue whenever there is a protracted diminution of secreting pressure in the secreting vessels—as happens in heart disease—and all the more easily if the actual number of the secreting vessels themselves is lessened. Wherefore, dropsy is wont to be the last event in the course of progressive contraction of the kidney, when, in consequence of this, the general nutrition, and so the vigour of the heart itself, is impaired. But so long as, in this common kidney affection, the energy of the heart as a pump is maintained, and the excessive diuresis commonly accompanying it continues, dropsy does not make its appearance, although the albuminuria may have endured for years. The dropsy in nephritis often forms the first symptom of the disease, and is more apparent early in the course of the inflammation than in its later stages, although the blood then, from protracted loss of albumen, must have become very much more deteriorated. The less the urinary secretion is, the more acute and the more copious will be the dropsy, and the latter will subside directly the water can again flow off by its natural channels.

The fluid which in renal dropsy passes through the walls of the capillaries into the subcutaneous tissue, and which accumulates in the serous cavities of the body, is a filtration merely of the blood serum through the walls of the vessels, and it constitutes a watery solution of salts exceedingly poor in albuminates. In the distribution of the exhalations in renal dropsy, the law which is observed is for the accumulations to take place, first and foremost, in the subcutaneous cellular tissues. It begins as anasarca, and, with rare exceptions, the dropsical effusions proceed next into the serous cavities, and subsequently invade the alveolar tissue of the lungs and the submucous cellular tissues. General anasarca may exist for a long time before dropsy invades one or more of the great serous cavities of the body. With some patients the first thing observed is a slight œdema of their ankles, noticed when they undress in the evening, at the end of their day's work; with others it is a swelling of their eyelids, which has taken place during the night, and remains when they get up in the morning. More rarely it happens that the first swelling shows itself in some other parts of the skin which present a loose and wide-meshed subcutaneous cellular tissue—as the prepuce and scrotum in men, or the labia pudendi in women. At first, and while the dropsy is still slight, in renal cases, these swellings exhibit

a remarkable fugitiveness and inclination to change their locality. The same individual whose eyelids, on getting up in the morning, were so swollen as to completely close his eyes, at night, after being about all day, opens his eyes with ease, but finds his feet are swelled; or, lying in bed and sleeping on one side, that side of his face on which he lay will, upon his waking, be considerably swollen, the other side being scarcely affected at all; and the whole condition will be reversed again upon his altering his position in bed. The parts of the body which are swollen and anasarcaous, in consequence of renal disease, are invariably strikingly pale, and the skin over them is, as a rule, quite remarkably dry and, in extensive dropsy, so stretched as to be, together with the fluid within it, quite transparent to transmitted light.

[*To be continued.*]

NOTE.—The text of the essays of Professor Carl Bartels, of Kiel, and Professor Wilhelm Ebstein, of Goettingen, in Volume XV. of the "Cyclopædia of the Practice of Medicine," edited by Dr. H. Von Ziemssen, has been followed as closely as possible, with additional notes from all the best authorities on the subject.

A. W. FOOT.

DERMALGIA FROM QUININE.

SOME additional reports of cases in which the administration of quinine has been followed by the occurrence of a cutaneous eruption have recently been brought before the profession. Dr. Field, Professor of Therapeutics at Dartmouth, U. S. A., reports in the *N. Y. Medical Record* (Nov. 30, 1878) the case of a young married lady, a sufferer from malarial poisoning, who could not take quinine. Her father, a physician of repute, had despaired of using quinine in her case. It made her feel, she stated, as if every drop of blood and every nerve were in her skin. There was great heat, oedema in some places, especially in face and hands, and great increase of cutaneous sensitiveness, all followed in a few days by desquamation of the cuticle. With these accidents were associated more or less of gastric disturbance, and, upon one occasion, violent and repeated vomiting, headache, and delirium. Dr. Field's treatment consisted in giving quinine in greatly lessened doses—a plan which he had pursued in cases of similar idiosyncrasy, and as the result proved in this case also, with success. The main action of quinine, according to Dr. Field, is exerted upon the sympathetic, and not upon the cerebro-spinal system of nerves. The dermalgia, urticaria, oedema, &c., which sometimes result from what should only act as a medicinal dose, serve, he believes, to illustrate and enforce this theory.

SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P.

VITAL STATISTICS

Of Eight Large Towns in Ireland, for Four Weeks ending Saturday, November 30, 1878.

Towns	Population in 1871	Births Registered	Deaths Registered	DEATHS FROM ZYMOTIC DISEASES							Annual Rate of Mortality per 1,000 Inhabitants
				Small-pox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhoea	
Dublin, -	314,666	635	741	29	7	22	—	10	35	6	30·6
Belfast, -	182,082	471	404	—	—	39	—	5	11	9	28·8
Cork, -	91,965	207	204	—	1	—	—	6	4	4	29·0
Limerick, -	44,209	66	75	—	—	—	—	3	1	4	22·0
Derry, -	30,884	59	53	—	—	—	—	—	—	1	22·3
Waterford, -	30,626	50	56	—	—	—	—	—	1	2	23·8
Galway, -	19,692	34	71	20	—	—	—	—	4	1	47·0
Sligo, -	17,285	21	17	—	—	—	—	—	2	1	12·8

Remarks.

An excessive death-rate prevailed in Galway, where 20 deaths from small-pox were registered. The mortality was also very high in Dublin, Cork, and Belfast; but in the other towns it was moderate, and very low in Sligo. The death-rate was 23·5 per 1,000 of the population annually in London, 20·7 in Edinburgh, and 26·1 in Glasgow. Within the municipal boundary of Dublin it was 32·8, and in the Dublin registration district—when we omit the deaths of persons admitted into public institutions from localities outside the district—it was 29·3. The deaths from zymotics were 131 in Dublin, or 7 less than a ten-years' average in the corresponding period (138·1). Small-pox, measles, and fever show an increased fatality, while scarlatina and diarrhoea were less destructive to life. Of the 35 deaths referred to fever, 11 were ascribed to typhus, 16 to typhoid, and 8 to continued fever of undetermined type. The epidemic of small-pox is again increasing in Dublin, owing to the continuance of cold weather and the consequently intensified virulence of the contagium in the badly-ventilated habitations of the poor. At the

close of the four weeks 86 small-pox patients were under treatment in the Dublin hospitals, compared with 47 at the close of the previous four weeks. In Belfast scarlatina began to decline in the commencement of November—the number of deaths per week being 16, 8, 8, and 7 respectively. Small-pox caused 28·2 per cent. of the deaths in Galway, where also 4 deaths were attributed to fever. The unusually low temperature of November largely increased—in fact, doubled—the mortality from respiratory affections in Dublin. The total deaths from this class of diseases were 203, compared with 102 in the preceding four weeks, and included 152 from bronchitis and 30 from pneumonia. The averages in the corresponding period of the previous ten years were—respiratory diseases generally, 123·6; bronchitis, 91·3; and pneumonia, 17·6 deaths.

METEOROLOGY.

*Abstract of Observations made at Dublin, Lat. 53° 20' N., Long. 6° 15' W.,
for the Month of November, 1878.*

Mean Height of Barometer,	-	-	-	29·898 inches.
Maximal Height of Barometer (on 19th at 9 p.m.),	-	-	-	30·469 „
Minimal Height of Barometer (on 10th at 3 p.m.),	-	-	-	29·200 „
Mean Dry-bulb Temperature,	-	-	-	37·4°
Mean Wet-bulb Temperature,	-	-	-	36·0°
Mean Dew-point Temperature,	-	-	-	34·0°
Mean Elastic Force (Tension) of Aqueous Vapour,	-	-	-	·196 inch.
Mean Humidity,	-	-	-	89·0 per cent.
Highest Temperature in Shade (on 10th),	-	-	-	50·5°
Lowest Temperature in Shade (on 26th),	-	-	-	26·3°
Lowest Temperature on Grass (Radiation) (on 26th),	-	-	-	22·5°
Mean Amount of Cloud,	-	-	-	50·0 per cent.
Rainfall (on 11 days),	-	-	-	1·338 inches.
General Direction of Wind,	-	-	-	N.W.

Remarks.

This appears to have been the coldest November experienced in Dublin since the year 1807, when the mean temperature of the same month was 36·8°, according to Dr. Kirwan's observations taken at Cavendish-row, Dublin. The mean temperature of the present month was 37·4°, or 6·4° below the average of the preceding twelve years (1866–1877, both inclusive). The cold period, which commenced on October 21, continued almost without intermission to the close of November. At first a wide area of low atmospherical pressure lay over Scandinavia and the Baltic, while numerous subsidiary depressions or bourrasques travelled across the United Kingdom from N.W. to S.E. Constant N.W. (W. to N.) winds were the consequence in Ireland and Great Britain. The latter country, being nearer the track along which the disturbances passed,

experienced heavy gales and an excessive rainfall. The E. coast of Ireland escaped with frequent frosts and a very moderate rainfall. On the 11th a severe snowstorm occurred over central and southern Ireland, and was followed by a keen frost next morning. Even at 9 a.m. the shade thermometer stood at 28.2° in Dublin. On the 15th and 16th a cyclone remained nearly stationary over Holland, causing strong N.E. gales and torrents of rain in England. On the 17th and 18th this depression filled up and its place was taken by an anticyclone, owing to which the N. winds died down to a calm, and the weather became frosty and foggy. On the 19th a thick smoke-fog over the city so effectually shut out the sun's heat that the highest temperature in Dublin was 36.3° . At Kingstown the weather was several degrees warmer—the thermometer rising to 45° in the shade. Another sharp frost on the 25th (min. in shade= 26.3°) was followed next morning by slight showers of rain and ice-particles, causing what is technically called a "glazed frost" and "silver thaw." Falls of sleet and snow occurred on the 27th and 29th, and were followed by drizzling rain and rather milder weather. Thunder and lightning prevailed between 2 and 3 a.m. of the 1st. Fogs were frequent from the 18th to the 26th. Solar halos were seen on the 9th, 15th, and 18th. There was a lunar halo on the 7th. Hail fell on the 4th, 11th, and 27th; snow or sleet on the 11th, 27th, and 29th. The rainfall in Dublin was below the average.

PERISCOPE.

Edited by G. F. DUFFEY, M.D., F.K.Q.C.P.

THE ACTION OF PLATINUM COMPOUNDS ON THE ANIMAL ORGANISM.

DR. F. KEBLER has studied, under the direction of Prof. Schmieberg, the action of platinum on the bodies of frogs, dogs, and rabbits. The compound used was platinum chloride ($2\text{HCl}, \text{PtCl}_4, 6\text{H}_2\text{O}$) neutralised with sodium carbonate. It was introduced into the lymph space in frogs, and injected directly into the blood in mammals. In frogs the results observed were the following:—Increase of general sensibility; sluggishness of voluntary movements; opisthotonic arching of the back when this was stroked, and, on stronger tactile irritation, reflex cry and spasmodic reflex extension of the hind legs; increasing paralysis of voluntary movements; spontaneous spasmodic contractions of the extremities, or of single groups of muscles; enfeebled muscular irritability; complete loss of motion; death. It is thus seen that platinum causes muscular paralysis, but, before this is complete, loss of *voluntary* motion is induced, which must be due to an influence on the nerve centres. At the same time the centres of co-ordinate movements are excited, so as to

cause spasmodic contraction, or even convulsions. In these respects the action of platinum resembles that of toxiresin or of digitaliresin. Death occurs before any appreciable effect is exerted on the heart.

In mammals the most marked effect of platinum was exerted on the intestines. In rabbits profuse diarrhoea occurred, and after death the stomach and intestines were found intensely hyperæmic. In dogs there were vomiting and diarrhoea, and considerable quantities of blood were passed *per anum*. The hyperæmia was found to extend to the entire abdominal viscera, including the kidneys, and in some cases the bladder was ecchymosed. The mucosa of the stomach and intestines was found to be swollen, hyperæmic, ecchymosed, and its surface covered by effused blood. These appearances resembled those witnessed in poisoning by sepsin and by arsenic. As in the latter case, it was found that after injection of platinum a steady fall of blood-pressure took place up to the time of death. This fall of pressure was caused by the paralysis of the abdominal blood-vessels, and due to an excentric cause, for the pressure could not be raised by irritation of the cervical medulla after section of the pneumogastriacs. Hence, either the muscular coat of the vessels was paralysed, or, more probably, a curara-like action was exerted on the vasomotor nerves. The pulse continued good up to the time of death, and the heart was unaffected. Besides the abdominal phenomena, paralytic symptoms were observed, apathy, increasing feebleness, and finally, complete loss of motion. This was due to central cause, as the irritability of the muscles was always unaffected. In rabbits shortly before death spasmodic contractions of the limbs were noticed, but which never amounted to actual convulsions. All these symptoms are probably, at least in large part, due to the anæmia of the nervous centres, caused by the paralysis of the abdominal vessels. The fatal dose of platinum in dogs was found to be not less than 5·6 mgrm., and in rabbits not less than 10 mgrm. per kilo. weight of the animal. These quantities are very close to the quantity of arsenic found to be fatal in the experiments of Böhm and Unterberger.—*Arch. f. experimentelle Pathologie und Pharmakologie*, IX., 137.

J. M. P.

APHORISMS ON CANCER.

IN a paper with this title, read before the Sixth Congress of German Surgeons in Berlin, Professor Esmarch detailed a number of interesting cases illustrative of the development of cancer from benignant growths, and of its treatment. He has met with four cases in which the malignant growths developed from atheromatous tumours of the scalp, and has repeatedly observed the cancerous degeneration of cicatrices, particularly those of lupus. The results of his observations have convinced him that a predisposition to malignant new growths is given in the hereditary scrofulous and syphilitic dyscrasias. In the treatment of desperate cases

that are too advanced to permit of removal by the knife, Professor Esmarch places most reliance on arsenic, which he employs both externally and internally. The fact demonstrated experimentally by Gaethjens, that the internal use of arsenic causes accelerated destruction of albumen and increased excretion of nitrogen, furnishes a rational basis for the employment of the drug in the treatment of malignant tumours. Professor Esmarch uses it to prevent recurrences after operation, as well as in cases that do not admit of operation. Externally he uses a powder, consisting of one part each of arsenious acid and sulphate of morphia, eight parts of calomel, and forty-eight parts of powdered gum-arabic. This powder is sprinkled thickly over the ulcer or wound every day; it exerts a powerful escharotic action, but is at the same time painless and destroys all offensive smell. Internally he gives Fowler's solution in progressively increasing doses. Latterly he has also followed out closely Beneke's recommendation with regard to diet. That author (*Deutsches Archiv für klinische Medicin.*, Bd. XV. S. 538) maintains that the quantities of albumen and of the phosphates in the blood are greatly increased in carcinosis, and that the tumours themselves are very rich in myelin, and hence forbids the use of all articles of food which contain albumen and phosphate of lime, confining the patients as far as possible to the hydrocarbons. Professor Esmarch gives a diet list, which is interesting, but which want of space compels us to omit. He has also obtained good results from Canquoin's chloride of zinc paste, but it is objectionable on account of the pain it causes. Electrolysis he has employed several times, but only once with any remarkable success. Iodide of potassium, given internally, in doses of ʒi. to ʒiiss. daily, and applied locally in powder, has, in his hands, proved successful in several cases of malignant neoplasms that in all probability had their origin in the syphilitic dyscrasia. In conclusion, he refers to the liability of confounding ulcerated gummy or tuberculous tumours with cancer, and mentions a case of *sycosis parasitica*, which was thought to be carcinoma until the microscopical examination revealed its true nature. In consequence of the difficulty of the differential diagnosis, he has made it a rule never to extirpate a morbid growth without previous microscopical examination to determine its actual character.—*Reprint from Langenbeck's Archiv*, Bd. XXII., Heft. 2.—*New York Medical Record*.

EFFECTS OF RHUBARB AND SANTONIN ON THE URINE.

DR. J. MUNK (*Virchow's Archiv.*, Bd. 72) found that after the internal administration of both rhubarb and santonin the urine was of a greenish colour, and that in both cases the addition of alkalies changes this to red. Notwithstanding this resemblance there are points of difference:—
1. Alkaline carbonates produce the reddish colour almost instantly after rhubarb has been taken, while after santonin the change is exceedingly

slow. 2. This change of colour of the rhubarb urine by alkalies is permanent; that after santonin passes away in twenty-four to forty-eight hours (if caustic soda be employed, it may last longer). 3. The rhubarb urine, coloured red by alkalies, is discoloured by digestion with steel filings; that after santonin is not. 4. By adding to the rhubarb urine an excess of baryta or lime water and filtering, the deposit retains the reddish colour and filtrate remains clear; on the contrary, in santonin urine the pigment remains in solution leaving us a reddish filtrate and an uncoloured sediment.—*Med. and Surg. Rep.*, Oct. 12, 1878.

ELASTIC CRAYONS OF NITRATE OF SILVER.

M. PAJOT (*Annales de Gynécologie and Gazette Obstet.*, No. 21) takes a laminaria tent two millimetres in diameter, dips it in thick mucilage, and then rolls it in finely-powdered, fused nitrate of silver, and allows it to dry. He thus obtains an elastic crayon of the ordinary size, which may be introduced into the uterus without fear of breaking. He believes this means to be applicable to other cavities, and for other more powerful caustics.—*Medical and Surgical Reporter*, July 27, 1878.

TINCTURE OF IODINE A SUBSTITUTE FOR QUINIA.

DR. FORDYCE GRINNELL, of the Wichita Agency, Indian Territory, has made an extensive trial of tr. iodi in malarial diseases and claims that "the results have been fully equal to those obtained from quinia." Dose—Ten drops, in sweetened water, three or four times a day, for adults.—*N. Y. Medical Record*, Nov. 30, 1878.

ELECTRICITY IN NEURALGIA.

As a guide to the proper current indicated in the various forms of neuralgia, Dr. Rockwell says: "I find the effects of pressure are exceedingly useful. I would not lay it down as a law, but it will be found, in the great majority of cases of neuralgia, where firm pressure over the affected nerves aggravates the pain, the galvanic current is indicated, while the Faradic current has the greater power to relieve when such pressure does not cause an increase of pain."—*Med. and Surg. Brief.* and *N. Y. Med. Rec.*

TREATMENT OF HYSTERICAL REFLEX-NEUROSES.

PROF. WEBER recommends the protracted employment of chloroform inhalations in the treatment of obstinate and severe cases of hysterical reflex-neuroses, of the respiratory apparatus, when the primary seat of irritation cannot be discovered and treated. He has himself proved the value of the inhalation in several cases. His first case was a lady with a spasmodic cough, that had proved rebellious to all treatment; she was cured in eight days by the chloroform inhalations, which were administered as often as the cough came on. A child with sneezing spasms was

cured in three days by the chloroform. Another lady with a spasmodic cough was treated with the same remedy for fourteen days, the inhalations being administered at first four or five times, and afterwards two or three times daily. She was much improved; the cough only came on after a walk, and the inhalations were only required then. In four weeks she was discharged cured; a subsequent slight relapse was cut short by the internal administration of chloroform. In the case of a girl, fourteen years of age, who suffered from spasms of sneezing, the inhalations produced a speedy cure.—*Memorabilien* and *N. Y. Med. Rec.*

A NEW MICROSCOPIC COLOURING SOLUTION.

SIGNOR A. TAFFANI says that the nuclei of cells, normal as well as pathological, may be coloured green in a few minutes with the following solution:—100 cc. of a saturated aqueous solution of picric acid, with 4-5 cc. of a saturated aqueous solution of aniline blue. The same colour may also be obtained by immersing sections, first in the aniline and then in the picric acid solution. Glycerine slightly coloured with picric acid preserves the colour of these preparations a long time.—*Lo Sperimentale* and *La Med. Contemp.* Nov. 1878, p. 462.

THE TREATMENT OF EARLY PHTHISIS.

DR. J. MILNER FOTHERGILL, in *The Practitioner* for September, 1878, discusses this important subject. The leading characteristics of early phthisis are cough, emaciation, night-sweats, and pyrexia, with more or less hæmoptysis, each symptom indicating an appropriate line of treatment. The most important indication is to arrest the night-sweats, and next to attend to the assimilative organs. If the night-sweats be not checked, the blood-salts drain out as soon as supplied. To arrest them there is no anhidrotic to compare with belladonna and its alkaloid atropia. Sulphate of atropia, in doses varying from a seventy-fifth to a twenty-fifth of a grain, is the best form in which to administer the drug. Marked dryness of the throat must be present before the effects of the drug become evident. Usually the first effects of restraining the night-sweats is to promote the appetite and digestion; and so long as hidrosis goes on it is useless to pour in milk, phosphates, or meat-juice; it is like pouring them through a sieve. Night-sweats come on generally in the deep morning-sleep; if awake, the patient does not suffer, while, on the other hand, profuse night-sweats commonly follow an opiate, given to relieve the cough. Sweating largely depends upon the relations existing between the pulmonary and cutaneous respiration, relations much more pronounced in human beings than is commonly supposed. When the respiratory centre is depressed in deep sleep, the sudoriferous glands are thrown into action, the blood being insufficiently aerated, and exciting their sensory nerves, and so producing cutaneous respiration or sweating. Belladonna

is a direct stimulant to the respiratory centre, and so, by stimulating this portion of the nervous system, does away with the necessity of hidrosis; hence it is well always to combine belladonna with morphia, when this latter drug is absolutely needed to relieve the cough of phthisis. Where belladonna fails, which is extremely rare, then oxide of zinc with henbane, or sulphate of copper with opium, is useful. Bathing with vinegar is also extremely valuable. An irregular practitioner in New York gained a great reputation by sponging with hot vinegar and capsicum powder. However attained, the first thing to be done is to arrest the night-sweats; and hot vinegar with cayenne pepper is useful in obstinate cases. Next in importance, the *primæ viæ* demand attention. It is more important to study the tongue in phthisis than the stethoscopic sounds. With a loaded tongue iron and cod-oil are wasted. Calomel with colocynth at bedtime, and a mineral acid with gentian or cinchona during the day, till the tongue cleans, are demanded. If the tongue be glazed, bismuth, with soda-water and milk, is indicated. Drains of all kinds are to be attended to. That from the skin has been disposed of; now comes diarrhœa, and this should be energetically combated, phthisical cases being soon depressed. In the early stage, half a grain of sulphate of copper, with one of opium, will often check it. Rice-water should be the beverage, avoiding beef-tea, which often sets up or keeps up a loose state of the bowels, owing to the excrementitious matter it contains. Leucorrhœa demands great attention; many cases hang on month after month unrelieved by treatment, because leucorrhœa or menorrhagia is unchecked. As regards hæmoptysis, we all know that this is not an uncommon end of a case of phthisis, yet in early stages it is often of great curative value, relieving local congestion, and being the starting point of recovery. When the stage of softening has been reached, then hæmoptysis becomes grave. At times a large pulmonary vessel is cut open, or an aneurismal dilatation of a pulmonary artery may burst into a cavity. Slight hæmoptysis in early apical consolidation is frequently arrested by a purgative. When cold hands and feet co-exist with hæmoptysis, warmth applied to the extremities, by relaxing the vessels, relieves the pulmonary congestion. The all-importance of pure air and good food are forcibly insisted upon. As regards the cough, hydrobromic acid with spirits of chloroform forms a better sedative than an opiate.—*London Medical Record*, December 15, 1878.

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PART I.

ORIGINAL COMMUNICATIONS.

ART. VI.—*Tremor as a Symptom in Disease.** By ABRAHAM COLLES, M.D., Univ. Dubl.

“TREMOR as a Symptom in certain Diseases” is a subject which has only in very late years attracted that attention in some of its branches which its importance would seem to deserve.

If we take a number of patients all suffering from this one common symptom of tremor, and examine them with a little care, we shall be struck by the fact that a rough division may be made of their cases into two well-marked classes—namely, those in whom the tremor is constantly and without intermission present during waking hours, and those in whom it is intermittent, and set going only under the influence of some exciting cause. A closer examination of these cases, however, will show that the members of each class present very many distinguishing features, and afford examples of very various diseases, so that we must allow that the division is after all only a rough one, in spite of the connecting link afforded by the presence of this one common and prominent symptom. This fact, however, will not, for the purpose to which I propose to apply it, render it either useless or invalid, as the point I wish to dwell on is the differing signification of the symptom when showing itself under its intermittent and under its continuous form.

Now, this difference in the exhibition of the symptom, though

* Being a Thesis for the Degree of Doctor of Medicine in the University of Dublin.

its existence has been long ago recognised (to a certain extent even so far back as the time of Galen), has yet never been till the last few years credited with the important significance which it in reality possesses, and no pathological difference was recognised as to the origin of the two conditions, and the *post mortem* appearances which are to be found in either case; and yet examination, if carefully conducted—either of the symptoms exhibited by the patient during life, or of the *post mortem* appearances which are to be found after death—will very clearly show us that the two conditions of tremor are most essentially diverse in their significance, and point to two totally opposite states of the nervous system.

Let me examine briefly the two conditions. The phenomena exhibited when the condition of continuous tremor is present are very well exhibited in the disease known as paralysis agitans, or trembling palsy, in which case the trembling continues without intermission during all waking hours, affording to the patient no intervals of rest or relief except when he is sunk in sleep. Now, here we have an instance of a disease which has been most unfortunately named, inasmuch as the case is in reality not one of paralysis at all, as we shall soon find by watching the symptoms which are displayed during its course; still worse becomes the name, and still more misleading, when changed, as is sometimes done, into senile paralysis, as the disease is by no means confined to elderly people (though certainly they are more frequently the subjects of it), but has been known to attack patients at very much earlier periods of life, in one case even as early as the age of sixteen, and I have myself at present a patient under observation suffering from this disease who has not yet reached the age of thirty-five.

Why, then, has the name “paralysis” been applied? When a patient suffering from paralysis agitans presents himself to our observation the symptom which chiefly arrests our attention is, as has been said, the presence of the continuous tremor; but a more attentive examination discloses to us the fact that there are other symptoms which accompany it, which, though less evident at first sight, are yet of no less importance, and their presence it is which gives to the case that deceptive appearance of paralysis which has for so long misled students of the disease. The chief of these symptoms are as follows:—(a) Excessive slowness in the execution of voluntary motions of the body or limbs; (b) considerable difficulty in articulation; and (c) a very marked peculiarity in gait, which is caused by a tendency experienced by the patient to fall either

forwards or backwards, the condition being known in the one case as propulsion, and in the other as retropulsion. But ought these symptoms to be regarded as in reality due to paralysis? Certainly not. The slow movements and the difficulty of speech are due, not to any weakening of the muscles at all, but to the existence in them of a condition of tonic rigidity. The muscles retain all their normal power, as may be clearly shown by the use of the dynamometer, which may even register an apparently abnormally high degree of pressure, but they are impeded in their action by the difficulty which has to be met of first overcoming the passive resistance offered by this muscular rigidity before voluntary motion can proceed, the patient being all the time perfectly competent to carry out (though slowly) any voluntary movement which he may desire. In some cases, however, there is another condition to be observed, preceding that spoken of, and very closely resembling it—namely, an apparent retardation in the execution of voluntary movements, causing their execution to be delayed, as it were, for a time after the mandate of the will has been issued, but without any attendant rigidity; but in these cases, as in those spoken of above, it will be found that in the execution of voluntary movements the muscles are capable, once this passivity is overcome, of exhibiting a perfectly normal amount of power, and thus showing that there cannot be said to be any symptoms of paralysis in its true sense present. The state of rigidity whereof I have spoken may be very well seen in certain cases, in which its influence is such as to cause even well-marked deformity of the limbs, and which in the hands has been not infrequently seen to produce a condition often simulating closely the deformities so well known as characteristic of chronic progressive rheumatism. This continuous and extreme tension of the muscles it is which gives rise to those sensations of painful cramp and ceaseless restlessness which prove often so distressing to the patient and so wearing to his attendants. Nevertheless, though I have stated that the disease is not paralysis, it is yet true that towards the close of a case we do not infrequently find a condition of exhaustion and marasmus supervening, which may be regarded as such; yet I cannot but look on such an occurrence in the light of a secondary affection, and class it among the many intercurrent affections to which these patients are liable, especially towards the close of the illness.

Before entering on the pathology of this disease, let me glance for a few moments at our second class of cases—viz., those in whom

the symptom of tremor is intermittent. Of this class a very typical example is afforded by the disease known as multilocular or disseminated sclerosis. Unlike the cases last considered we find in them the tremor showing itself only under the influence of some stimulus. While the patient remains quiescent the symptom continues in abeyance, and it is only when the execution of some voluntary effort is attempted that an attack of tremor is produced in the affected muscles, assuming a more or less severe form according to the stage of the disease, and subsiding again when the patient resumes the state of passivity. Passing over other concomitant symptoms as not bearing any direct relation to that under examination, I would direct attention to the very marked paresis which shows itself even in the very early stages of the disease, and which, as a general rule, may be said to make its appearance first in the lower extremities, spreading at a later date slowly, though surely, to the upper extremities and body. This loss of power is not infrequently the very first symptom which is noticed by the patient, giving rise to a sense of weariness and weight in the *lower* limbs and gradually affecting other parts of the body. It is due from the outset to an absolute loss of power, but is never accompanied by any muscular rigidity till towards the close of the illness, thus differing remarkably from the conditions found in paralysis agitans, in which I have shown above that these conditions are absolutely reversed.

As in all cases of both these classes treatment has proved to be almost totally ineffectual, their chief interest for us lies in the pathological significance of the prominent symptom of which I have been speaking, and it is to this aspect of them that I would especially wish to allude. Of the pathology of the first class of cases we must, I think, be content to say that it is a neurosis—i.e., that we are as yet unable to detect in them the occurrence of any distinct pathological change in the nerves or nerve-centres. In making this statement I exclude from consideration the few cases which have been reported by Bamberger, Skoda, and others, where lesions of various kinds have been described as having been found, these lesions having been so inconstant and so varied that I would consider them rather in the light of complications than as essential conditions of the disease. Very different, however, is the state of things disclosed by a *post mortem* examination in a case of disseminated sclerosis. Here we find disease of a very definite and uniform character. Sclerosed patches are found more or less extensive, in

which, as the disease advances, nerve cell forms and trabeculæ disappear gradually, while there is a corresponding increase of fibrillar bundles, which, by their increasing pressure on the nerve fibres, cause them to waste more and more, and to lose at a comparatively early stage their outer sheaths by a process of gradual absorption, though the central axis is found to persist for a considerable time, appearing capable of resisting for a lengthened period the pressure to which it is subjected, without entirely disappearing or even losing altogether its power of performing its functions. These sclerosed patches may be found in any part of the nervous system—the brain, spinal cord, or nerve trunks—showing, wherever situated, a constant tendency to spread, not only by enlargement of existing patches, but by the appearance of fresh ones in other parts of the nervous system, till the scene closes with general paralysis and death.

Now, what I would point to as the value of this symptom in these cases is this—that, in the first instance—the continuous palsy as seen in the disease known as paralysis agitans—the presence of tremor is to be traced to central nerve *irritation*, without any true paralysis at all, but accompanied by well-marked tonic contraction of muscles; while in the second case—that of interrupted palsy as found in multilocular sclerosis—it is to be traced to a true central paralysis, resulting from destruction or disease of nerve matter. That this is the relative value of the symptom in its two manifestations may be further seen from a consideration of the following facts. We know that in cases of paralysis unaccompanied by any central irritation, peripheral lesions are never found to occur, except as purely secondary complications—*e.g.*, muscular wasting will not be found to occur primarily, nor till the illness has continued for a long time, when wasting may make its appearance as a simple result of the prolonged want of use of the muscles. Sores, again, are not found to form as a direct result of such disease, except in so far as they may be produced by mechanical means, which may the more easily happen in consequence of the decreased sensibility of the parts. Now, this is diametrically the opposite of what we find occurs in cases where central irritation exists. Thus rapid wasting of the muscles may not infrequently be seen as an early and well-marked symptom accompanied by cutaneous and other peripheral lesions. In applying these facts to the cases before us we shall find that they tend to bear out what I have said with regard to them. In those in which I have spoken of the existence of paralysis, we never find these peripheral lesions showing

themselves as a normal element of the disease, but only as late and secondary complications; while in the other cases where I have spoken of the existence of central nerve irritation, the occurrence of some such lesions may be looked on as almost a certainty. I have now myself under observation a case of considerable interest in its bearing on this point—that of a man of advanced age who has for many years been suffering from paralysis agitans. He is subject to periodical exacerbations, or one might almost term them acute attacks of the disease, and the curious point in his case is that at the times when these exacerbations occur an erythematous eruption is found simultaneously to make its appearance on both legs, which gradually dies out again as the severity of his other symptoms abates. Were the case truly one of paralysis we should not expect to find any such accompanying lesion.

From these considerations we may, I think, feel justified in saying that the relative value of the two forms of tremor as a pathological symptom is this:—That when the trembling is constant the case is to be regarded as one of irritation of the nerve centres (though the exact nature of this irritation still remains to be determined), while in those cases where the tremor is intermittent and excited chiefly by attempts at voluntary motion, we have to deal with instances of real paralysis, due in all probability to nerve mischief deep-seated, and, so far as our present knowledge extends, irremediable. Both classes of case present very much of interest, and a wide field for research and study, not only to the pathologist, but also to the physician; and, while regretting the very small amount of real knowledge of the subject of which the profession can at present boast, we must hope that, with the daily advance of science, we may soon obtain clearer and more definite information for our future guidance.

ART. VII.—*Symmetrical Corectopia with Dislocation of the Lens.*

By H. MACNAUGHTON JONES, M.D., M.Ch.; F.R.C.S., Ire. and Edin.; Professor of the Queen's University, Ireland; Surgeon, Cork Ophthalmic and Aural Hospital, &c.

THE occurrence of this congenital anomaly is, in my experience, very rare. Of necessity my field for observation is comparatively limited in a provincial town; but, if not uncommon, I should have been sure to have seen, in many thousands of ophthalmic cases, or met in general practice, a second case of the nature here depicted,

which I have not. This has tempted me to place it on record, though I am aware that from time to time many analogous cases of eccentric pupil have been noticed, and may be found referred to in reports and ophthalmic works.

Family history.—Father and mother healthy; sight good; father addicted to drink. Eldest son in the army; sight perfect. Second son a shoemaker; sight perfect. Two younger children; in one there is extreme near sight, in the other the vision is excellent.

T. S., age eighteen. Congenital luxation of left lens outwards, lying half across the pupil; congenital luxation of right lens upwards. In both eyes the iris is tremulous. $V. = \frac{1}{60}$; with -2 sph., $V. =$ little less than $\frac{1}{3}$.

Brother, age twenty. Pupils as represented in drawing (Fig. 1).

Fig. 1.

$V. = \frac{1}{3}$; hardly improved by spherical concaves; both acted perfectly to atropia and eserin; periphery of the lens crossed the pupil at about $\frac{1}{3}$ from the outer margin, when dilated with atropia. From this I judged that the lens lay nearly, if not quite central in the eye. There is a well-marked "crescent" in either fundus.

Fig. 2.



The other drawing (Fig. 2) is of a little girl, age thirteen. Extremely hypermetropic; when about seven years of age I gave her convex glasses. I then lost sight of her, and she only again came to hospital ten days since. I dilated the pupil (with liq. gelsemiæ) to demonstrate the displaced lenses to some students, when this drawing was made; both lenses are displaced upwards and fixed; there is no tremulousness of the iris.

ART. VIII.—*Trichorexis Nodosa*.^a By W. WHITLA, M.D.,
Physician to the Ulster Hospital for Children, &c.

A DISCUSSION has been going on for some time in *The Lancet* and other medical journals under the name of "Piedra," and I notice that a lively interest is taken in it in England. Last summer M. Desenne described for the first time a condition of the hair found in the beards of the Columbians, and exhibited specimens before the Academy of Medicine. The diseased or affected hairs were seen to have distinct nodosities at intervals upon their shafts like so many small beads, but which were fixed to the hair, and in some cases only surrounding the shaft partially. The peculiarity of the nodosities, which distinguish them from growths at all approaching them in microscopic or naked-eye characters, is a very remarkable one. They will scratch glass by reason of their stony hardness; and when a bunch of the affected hair is struck against any hard surface a distinct ring is heard. M. Desenne found satisfactory evidence of spores, and concluded that the disease was evidently parasitic. A short account of his paper appeared in *The Lancet* of August 3rd, 1878, under the heading "A New Parasitic Disease of the Hair." It is not my intention to detain you with any remarks upon this singular and rare disease, which is, as far as I know, peculiar to Columbia, and has only been examined by a fortunate few, who are by no means at one about its pathology. The notice of M. Desenne's paper was the signal for many observers to write—some agreeing, some pointing out differences in their observations; and, upon a careful perusal of the recent literature of the subject, there can be no doubt whatever that the English observers describe a totally different affection which has been for many years recognised in this country by a few. It is of this second condition, called by many names, such as "*Trichorexis Nodosa*," "*Fragilitas Crinium*," and "*Clastothrix*," &c., that the specimens exhibited under the microscope are examples. I noticed it about six years ago, and found its histological characters so beautiful and regular that I have watched for it and examined many such specimens. The first published account of it that I am aware of is from the pen of Erasmus Wilson, in the early edition of his work upon "*The Healthy Skin*," where, as early as 1849, he described accurately this state of the hair. In 1855, Beigel independently studied the disease, as he called it, of "bursting of

^a Abstract of remarks made before the Ulster Medical Society, 7th January, 1879.

the hair." In 1857, Dr. Wilks, in his lectures on "Pathological Anatomy," figured and described the state of the hairs; and later, Tilbury Fox, under the head of "Sycosis," figured a similar state of matters, plus a graphic drawing of sporules mixed up with the fibres. A few other published references to the subject may be found, but its history and literature are ably and exhaustively recorded in *The Lancet* of December 7th, 1878, by Dr. Thomas Fox, Physician to St. George's Hospital—who, by the way, quotes Dr. H. S. Purdon as describing an affection which he was not at the time referring to.

Now, as regards the condition or affection, or "disease," as it has been called, its naked-eye characters vary slightly in differently coloured hairs, and I may say here that it is solely to specimens taken from the hair of the face that my remarks refer. The hairs affected are marked at irregular intervals with white transverse bands, which thus resemble somewhat minute porcupine's quills. The band is of a pure clear white in the hairs of an amber colour; in the long dry and fine hairs, seen in dark reddish-brown beards, these transverse markings have a dirty white or translucent hue, which often renders them difficult of detection. These bands, or white transverse striæ, have been called nodes by most observers, who describe them as if the swellings of the hair which constitute them were quite perceptible to the naked eye. Such is hardly correct, as in most cases no one could possibly recognise them as swellings till under the microscope—persons with very sharp sight might sometimes.

The hairs breaking off generally at one of these nodes possess thereby another peculiar appearance—their ends seem white and abrupt, just as if they had been burned, and once I was consulted by a gentleman for this state of the hair whose family physician persisted in telling him that his beard was "singed." There may be only one white mark on a single hair, or there may be twenty. This latter condition is very rare, and the number of nodes seem to depend greatly upon the coarseness or fineness of the shafts. The strong thick hairs of curly beards and whiskers break off short at these nodal spots, and seldom show more than six upon the stump of the shaft remaining, while the finer sorts of hair, which are straighter, do not give way so readily, and upon one patient's beard, which I will exhibit to you, you will find hairs eight inches long, with from six to eighteen nodes visible to the naked eye. His case is exceptional, however. As to the situation of these

markings on the shaft, I have seen one upon a long hair within one-third of an inch from the follicle, the remaining part quite normal. I have also seen one nearly as close to the free end in a similarly healthy shaft; between these two are all possibly divided conditions. The shaft is frequently bent at the node, the angle formed being always a sharp one—in very curly hairs it may be acute. There is not much bending (sometimes not any) at the junctions of the nodes in long, straight, fine hairs.

The microscopic characters of the affected hairs are very striking, as you will see by the accompanying diagrams and under the different microscopes upon the table. The fibres of the cortex of the shaft are frayed out in a beautiful manner at the node. The hair seems just ready, as it were, to break into two distinct pieces through the centre of the swellings, as many of the circumferential fibres are entirely separated. The diagram which I draw of the appearance seen under instrument marked No. 1, you will observe, is almost a *facsimile* of Mr. Wilks' woodcut in the number of *The Lancet* on the table. The appearance perfectly resembles two blunt camel's hair pencils placed end to end, and gently pressed against each other so as to cause the hairs at the circumference to bulge out freely. The medulla is seen in No. 2 to be absent entirely, but under the next instrument you will observe a node just in the act of forming. The specimen proves the error of a very eminent observer, who denies that there is any swelling prior to the fraying out of the fibres. Here the diameter of the shaft is unmistakably increased. Only a few small fibres have started out on each side of the node, the soft cells of the pith are much increased in number, and the pigment is scanty. Under another instrument you will see a young node, which, however, is perfectly formed; the small, bright, scale-like particles inside the long frayed-out fibre are, I believe, the pith-cells which have dried up to some extent and are adhering to the inner surfaces of the fibres, possibly showing the reason of the varying quantity of the medulla, as noticed by Dr. Thomas Fox and Kaposi—the drying process probably commencing when the outer fibres burst open. Pigment varies; as a rule you see scarcely any in the outer layers of fibres, and seldom any in the central part, occupied by the medulla, in the node. Air cavities are seen, but only rarely, in the medulla of the shaft at a distance from the break, but sometimes they are found near to it, and, on close examination, you will find this is owing to the consequent drying of the soft cells or

the mechanical separation of the harder ones. Some of these hairs are in every other respect normal and perfectly developed, others again are badly formed, stunted, devoid of epithelium, and brittle; at the same time hairs of this latter class are found with all these imperfections, but without any trace of the characteristic nodes. I cannot see any departure from the normal state in the root-sheath. As regards longitudinal splitting, associated with trichorexis nodosa by some observers, I have frequently seen it, but as often in sound hairs as in those affected with the transverse markings. I have never been able to see any evidence of parasitic disease, and the different specimens before the Society are devoid of any resemblance to spores.

The most interesting feature in connexion with this state of the hair is its *distribution*, and it is to this point I wish particularly to direct the attention of the Society. I have failed to find it in about half a dozen men whom I have imperfectly examined, and whom I only saw either in a bad light, or rather hurriedly. I am almost certain it is there, but cannot state it is positively. I have failed to find it once when I had every facility and sufficient time, and spared no trouble in searching. If you permit me to make this solitary exception, I say I have seen it in 200 or 300 men whom I have examined, and I believe within certain ages the distribution of these characteristically marked hairs is as universal as the growth of hair itself. The specimens upon the table are each, as you will see, from the office-bearers of the Society, whom I had an opportunity of examining during the last week, beginning with the President. The single exception above referred to was in the case of the Treasurer, who has the most perfectly formed beard to be found, and his case abundantly proves the rule. After a search I came upon a doubtful hair, and placed it under the microscope; it had no characteristic markings, but, what was to me more interesting, a distinct though very small swelling at one portion of the shaft, with two fibres frayed out, as you see in this drawing on the black board, and its owner was convinced that this was the first step in the process of bursting; it was not, however, anything like so evident as the specimen under No. 3. If this be an affected hair, then I have not seen a beard where I have carefully examined and failed. In all doubtful cases the hairs are tested under the microscope. I examined a chemist three times carefully and failed to find a single hair affected, and he was put down as the only exception I had then met with. He had most exuberant growth

of beautiful whiskers. I met him a short time ago; he had got his whiskers closely trimmed. He readily consented to re-examination. The long flowing hairs were not in my way now, and I soon pounced upon two beautiful specimens, one of which is under No. 5; these were the only hairs affected, I believe, upon his entire face. One other case, and we have done with this part of the subject. I sent, some time ago, a few hairs to a celebrated histologist. He examined them, and concluded that the alterations were the result of chemical changes produced by hair-dye. When I next met him I told him I had been finding them in everybody. He seemed much amused, and, when I suggested the possibility of their being in his own beard, he laughed outright. "He had examined it often for other purposes," but upon importuning him I was permitted to look, and found them in thousands. I mention this to show how they may be overlooked unless specially hunted for—the gentleman referred to being the most accurate and gifted observer that it has ever been my good fortune to meet. They will be found studded thinly over the entire beard. This is uncommon. You will often see them in patches as large as a crown. Their most common site is just under the lobule of the ear. Another very favourite situation is over the region of the depressor anguli oris muscle. They are *very seldom found* in the much-twisted and incessantly-pulled mustachios of young men. I have only met with them there once or twice. I have seen them in gray-haired men of sixty-six and in boys of twenty. It has never been my lot to find them in the hair of the scalp or on that of any other region of the body.

The rarest class of cases is that where the hairs of the face seem all affected uniformly, and this is the only way in which I can imagine that the matter has been spoken of as being so rare and unusual. Of the last class of patients I have only seen two well-marked cases—one of whom I now show you. He is the owner of an extensive bread-baking concern, and has kindly consented to show himself. You observe the general character of his beard; it is long, straight, red in colour, and surprisingly dry. You feel it and you almost think you are grasping some dry flax, and he tells me he washes himself every morning, beard included, with several lathers of soap, and dries with a coarse towel. He certainly seems to favour the idea that this is a mechanical process operating upon dry hairs. But, upon the other hand, look at these beards round the table shining—some of them even glistening

with sebaceous material; they also show the same state of matters to a less extent. Some of these dry hairs are under the last instrument; they in no way, that we can see, differ from those of the chemist's case. I regret to say that I have not yet been able to have an examination of the hairs upon this patient's pubis. A peculiar feature I have noticed in those cases where the affected hairs are found in patches is a sense of burning itchiness, or even smarting, experienced in the patches at times, especially when the face is flushed. This, at one time, led me to think that some irritation might be going on in the hair follicles, and the presence of the *entozoon folliculorum* suggested itself to me, but I could not satisfy myself that it had any connexion with it, and I have not been able to transplant the animal to the pubic region. This brings us to the pathology of the subject, and it is possible that we have something still to learn as to the cause of this singular state of the hair. When I first saw it the idea of mechanical twisting being the *rationale* seemed obvious, and I marked certain hairs close to the affected ones upon my own face, and pulled at them for a few weeks without producing any effect whatever. This, with the fact of their being so rare in the hair upon the lip, satisfied me that torsion had nothing to answer for. One of the specimens upon the table would at first sight lead you to believe that this theory was correct. Here a transverse cleavage has not yet occurred, but the shaft of the hair is bent round like the arch of a bridge; upon the convex surface, where the greatest strain would be, the fibres have sprung out, as if ruptured by the bending. That this is not the beginning of the process, you have only to look at the concave side, and, though you find no frayed-out fibre-cells, you will see a distinct preliminary bulging, owing to the proliferation of the cells of the medulla. What has occurred is evidently as in No. 3, and if you suppose the specimen there bent at the enlargement you would have this condition precisely repeated, showing that the bending is simply secondary to the swelling and consequent weakening of the shaft at that place.

Beigel states that the breaking is caused by the expansion of air imprisoned in the shaft which, finding no egress, bursts the fibres of the cortex. This could only, however, occur as the result of an explosion, for, suppose one fibre gave way, a chink would be formed and the imprisoned air would escape; and, indeed, I see no reason whatever for supposing air acts in any way inside the cylinder. I say this without hesitation, though against so distin-

guished an authority, because the idea presented itself to my own mind long before I had heard of his publication, and I had also discarded it. It has been suggested that altered nutrition, caused by affection of the trophic nerves, as in alopecia areata, would explain this; but it is hardly possible to apply this explanation to cases where only a few isolated hairs are found in a healthy beard. The parasitic theory will not satisfy those who fail to see any evidence whatever of spores; and when you think of the beautiful sieve which the finely reticulating fibres of the cortex make, and consider that this sieve may remain open to every blast of air which blows upon the face, the wonder is that it does not exhibit from time to time quantities of living organisms.

The universal distribution of "*trichorexis nodosa*" suggests to me that it might, strictly speaking, have no pathology. Can we call a condition which is so general a "disease?" One is led to regard it rather as the result of some physiological action modified or varied perhaps in different individuals. I do not willingly submit another to the many explanations given by writers upon this subject; but the view I would lay before you is one which seems to me the most *likely* (that is all I can say for it), till some more satisfactory one is presented. It is this—that the outer fibre-cells of the cortex of the shaft of a growing hair, becoming hard and firm as do those of the nails, do not yield to the expansion of the inner quickly-growing soft cells of the medulla, and the result is their gradual separation and the disintegration of the cortex structure. The specimens exhibited tend to support this. You have unmistakable evidence of a prior enlargement. Abnormal dryness may make these cells more prone to split up; and I found them more abundant in the cleanly, who freely use soap and water, than in those who pay no attention to their personal appearance; but why the cells should split up in a patch upon one side of the face and remain normal on the other, is not easily explained. There is no state of the system which I could notice that favours their appearance. They do not seem to be at all influenced by the broken-down health consequent upon recovering from typhoid and other fevers or lingering diseases. They will be found in the weak and robust, the young and the old, the vegetarian and the gourmand, the teetotaller and the drunkard.

ART. IX.—*Sloughing Sore Throat.*^a By FRANCIS PEIRCE, M.D.,
Univ. Dubl.; F.R.C.S.I.

“SLOUGHING sore throat,” described by Churchill under the name of “putrid sore throat,” appears to me to be a disease of more frequent occurrence than is generally thought. During the greater part of last year I had many cases, scattered all over my district. They were characterised by the general symptoms of high temperature (often over 102°), quick pulse, and frequently great prostration of strength; urine sometimes albuminous. At first there was nothing to attract attention locally beyond the appearances of ordinary sore throat, but on the second day or so a white, or rather gray, spot was to be seen—not merely a patch on the fauces or uvula, as the case might be, but the tissues themselves changing colour—becoming, in fact, gangrenous. The sloughs would separate about the sixth day, leaving a sluggish ulcer, with evident loss of tissue. In some of the cases the submaxillary and parotid glands were inflamed, while, on the contrary, in many where the disease assumed the most malignant type, there was no glandular complication whatever.

I am aware that the above disease would be classed as diphtheria by many practitioners, or, if they did not call the affection by that name, they would style it diphtheritic sore throat, which means pretty much the same thing to the general public. I look upon the total absence of false membrane, the very frequent absence of glandular inflammation, and the fact that the disease confined itself, in its malignant form, almost altogether to children, as quite sufficient to absolutely distinguish it from diphtheria.

The above-described infectious disease assumed the form of an epidemic last year in the Upton district of the Hundred of Wirral, Cheshire; and, although many persons were seriously affected, all recovered well and rapidly, till Hoylake became the seat of its operations. There was nothing unusual about the first cases which occurred in the outlying parts of this village; then a child living more in the village was seized with the ordinary symptoms, which rapidly became malignant, and terminated fatally. Within a few weeks eight children died similarly in the only three houses which were infected. After the first fatal case all the children affected died—generally between the fifth and tenth days. The mother of one of the families was the only adult affected, and, although there

^a Being a Thesis for the Degree of Doctor of Medicine in the University of Dublin.

was a considerable slough in her case, I could not prevail on her to remain in bed more than one day. She made a good recovery. I feel confident the malignant type assumed by the disease was caused by the extremely bad sanitary condition of the locality, which is altogether drained on the dry-well system—in other words, the houses are built on a cesspool covered by a few feet of sand.

The epidemic then passed on to West Kirby, a village about one mile from Hoylake, equally deficient in sewerage, but blessed with many natural advantages. There the mortality was but slight. There were two adults amongst those affected—one, a delicate girl of about eighteen; on the third day nearly the whole of her uvula sloughed away, but she made a fair recovery—the other, a young gentleman in the last stage of consumption, who rapidly succumbed to the disease—this being the only instance proving fatal in the adult.

The treatment I found most efficacious consisted in the administration of plenty of beef tea from the commencement, and, as the sloughs came away, the free use of stimulants. The only internal medicine that appeared to me to do any good was the tinct. ferri perchloridi, given in large doses. A gargle containing chlorine gas corrected the unpleasant smell and seemed to improve the condition of the parts.

ARNICA IN BOILS.

DR. PLANAT, of Nice, claims that arnica has the power of aborting an eruption of boils with extraordinary rapidity, except when due to diabetes. His method of employing it is very simple. In order to render its action on the small vessels more energetic, he applies it directly to the inflamed spot, in the form of an ointment, of which the formula is as follows:—Ext. of fresh arnica leaves, ʒiiss; honey, ʒvss. If the mixture is too fluid, he added powdered lycopodium, or some other inert powder, until it acquired the proper consistency. It is then spread pretty thickly on a bit of oiled silk or diachylon plaster, and applied to the boil. It is rarely necessary to renew the dressing more than once in twenty-four hours. As a rule, two or three dressings are enough to make a furuncle abort. A curative action is also obtained by the internal administration of the drug. Dr. Planat gives 3 to 4 drops of the tincture, largely diluted, every two hours; and he has seen the furuncular eruption disappear very rapidly under the treatment.—*St. Louis Med. Jour.*

S. W.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Diphtheria: its Nature and Treatment, Varieties, and Local Expressions. By MORELL MACKENZIE, M.D., Lond London: J. & A. Churchill. 1879. 8vo. Pp. 104.

THE appearance just at the present time of this review of the history of an affection which has cast so dark a shadow over the entire nation is opportune; but it is doubly so as coming from the pen of one who has had, probably, the largest experience of all forms of laryngeal disease of any physician in this country. And as the labours of the Committee appointed to inquire into the etiology, &c., of diphtheria, have just terminated—the result of the inquiry being embodied in a Report to which we shall presently refer—it is as well that, in following the discussion on that Report, we should have in our hands so complete a *résumé* of the entire subject as that given us by Dr. Morell Mackenzie. The work is divided into eleven chapters, which embrace every point of importance, from the history of the disease to the secondary consequences which attend it in many instances. The definition of the disease, as given by Dr. Mackenzie, is as follows:—

“Diphtheria is a specific communicable disease, occurring epidemically, endemically, and solitarily,* and characterised by more or less inflammation of the mucous membrane of the pharynx, larynx, or air-passages, and by the formation on the surface of those parts—especially on the mucous membrane of the fauces and windpipe—of a layer or layers of lymph or false membrane, generally showing signs of bacteroid mycosis. During an epidemic other mucous surfaces exposed to the air, and wounded surfaces of the common integument occasionally, but less frequently, become covered with a layer of lymph, subsequently to, or independently of, a formation of membrane in the more ordinary situations. The disease is generally of an adynamic character, is often associated with a disturbance

* The author uses this word in preference to the term “sporadic,” which he observes is commonly employed in connexion with diseases supposed to be of spontaneous origin, or, at any rate, is applied to those which it is presumed arise from accidental causes, independently of any contagious influence.

of the renal function (albuminuria), and is frequently followed by lesions of innervation rarely giving rise to permanent paralysis. The symptoms as regards respiration, vocalisation, and deglutition, vary with the site of the disease. By far the larger proportion of fatal cases terminate by gradual apnœa, but a certain percentage sink from asthenia, blood-poisoning, and cardiac thrombosis."

As is well known, the origin of the term diphtheria is associated with the name of Bretonneau, who coined the word "diphtherite"—Greek, *διφθέρα*, a skin or parchment, and *ἵρως*, hasty—to indicate its inflammatory nature. Trousseau, following, used the term "diphthérie;" and hence the word, as used in British nomenclature, "diphtheria." In Chapter I. we find many interesting facts connected with the earlier attempts at a description of this or a similar disease. Dr. Mackenzie quotes from D'havantare, an Indian physician, who wrote a systematic work on medicine in Sanskrit, a Latin translation of which by F. Hessler, published at Erlangen in 1844, is in the British Museum. He describes a disease in which "an increase of phlegm and blood causes a swelling in the throat, characterised by panting and pain, destroying the vital organs, and incurable." He also says "a large swelling in the throat, impeding food and drink, and marked by violent feverish symptoms, obstructing the passage of the breath, arising from phlegm combined with blood, is called 'closing of the throat.'" The probability of the recognition of the disease by Hippocrates, the Syriac ulcer of Aretæus, the description of the barking cough and other characteristic features, such as the lividity and subsequent paralytic states, by Cœlius Aurelianus, are referred to.

It would appear as if the Askara of the Talmud was identical with diphtheria. In a footnote we find that—

"The word 'Askara' (אֲסָרָא or אֲסָרָה) means literally 'closure,' and is allied to the word Sakar (סָכַר), 'to shut up' or 'to close.' Askara is frequently used in the Aramaic dialect of the Babylonian and Jerusalem Rabbins. Its effect is compared to strangulation, and its danger consists in its being communicable to others. Some modern lexicographers translate 'Askara' by 'Croup.' Buxtorf, in the Basle edition of the Talmud (1639), renders it by 'Angina.'"

For the perusal of the interesting sketch which follows, of notices of the disease in Holland in 1557, by Peter Forest and Von Woerd; by Baillou in France, and Villa Real (1611) in Spain; Sgambatus, Severino, and others, in Italy; Patrick Blair and Fothergill at

home; and so down to the period when, in 1765, Home more fully described the disease, we must refer our readers to the sketch, so condensed, and yet so perfect, contained in the first chapter. The earliest American record appears to have come to us in 1789, from Philadelphia, from Dr. Bard, to be followed in 1798 by a paper by Dr. Archer, of the same city. In France the principal names associated with diphtheria are those of Marteau de Grandvilliers and Chomel, in 1749; and later, in connexion with the prize offered by Napoleon I., Albers, Jurine, &c.; to be followed by the memoirs of Bretonneau in 1818 and 1826; and, still later, Trousseau. Previous to this, Cheyne and Cullen, in Edinburgh, distinctly portrayed the features of diphtheria; but all through, and to quite a recent date, the diseases croup and diphtheria were considered by most authors as distinct diseases. The past quarter of a century has seen a mass of literature published on the etiology and pathology of this disease. Most prominent are the descriptions by Ernest Hart, Sir William Jenner, Greenhow, Thursfield, George Johnson, and the perfect account by Lewis Smith, in America, in his work on "Diseases of Children;" and the writings of Rindfleisch, Oertel, Hueter, Nassiloff, Wagner, Eberth, Labadie-Lagrave, and many others on the Continent.

In dealing with the etiology of the disease, and the natural history of the contagium, and subsequently in treating of the pathological changes in the mucous membrane, the views of Laycock, Erfurth, Bühl, Hueter, and others, are discussed. Oertel's name is that best known in connexion with the bacteria theory, which would assign to the presence of the micrococcus, or spherical, and the bacterium termo, or rod-like bacterium, the chief place as a cause of diphtheria, thereby stamping the disease as having, in the first instance, a purely local origin, there being a specific virus acting on the throat. But within the past few years this idea of Oertel's has not been sustained; and though, doubtless, it is true that bacteria are present in numbers on the fauces, on the lips of the tracheal wound after tracheotomy performed for diphtheria—in sores elsewhere than in the throat after surgical wounds—yet their mere presence does not prove Oertel's view. Bacteria, such as those described (*leptothrix buccalis*) on diphtheritic membranes, are found in various forms of simple or specific stomatitis, and in other conditions altogether removed from a suspicion of diphtheritic infection. According to the researches of Dr. Beale, vegetable organisms are present in the tissues of both man and animals even

in health, and may be found in the tongue, and elsewhere in the digestive tract. But, in addition, clinical facts would appear to disprove altogether the exclusive theory of the parasitic origin of diphtheria. Long before any local symptoms are developed, the system has given evidence of general disturbance; nor indeed is it rare to find cases without marked local symptoms from first to last. Such a case we have at present under our care, in which a state of phlegmasia in one leg has supervened on the fever of diphtheria, with no local sign save a few small pustules, general redness of the fauces, and a thin pellicle in a few places on the palate.

Yet the bacteria may circulate in the blood, and there may be no local appearances; but, as a strange coincidence of diphtheritic states, we notice the immunity which the lungs frequently enjoy from the attacks of the specific poison—a fact, as pointed out by Lewis Smith in his admirable work on “*Diseases of Children*,” in the chapter on Diphtheria, which invalidates the theory of the bacterial origin of the disease, as pneumonia, he says, is a rare complication. And we may conclude with Lewis Smith that “the truth regarding the relation of bacteria to diphtheria lies in one of two hypotheses—either that these parasites are the specific virus, and therefore cause the disease, or that the cause is something more subtle not yet discovered, which so alters the tissues and the blood that they become a nidus in which the bacteria are early and quickly developed, so that from being few and innocuous in the system, they occur in myriads.” Dr. Lewis Smith’s investigations into the pathological changes in the diseased membranes lead him to adopt the latter view.

This is hardly in accord with the experience of Dr. Morell Mackenzie, who lays special stress on the exudation changes which occur in the lungs, chiefly of a broncho-pneumonic nature; nor indeed can we say it agrees with our own, as we have frequently found, from the second to the sixth day, extensive lung changes accompanying or preceding the full development of the diphtheritic membrane. Beyond question, as in the interesting case of Dr. Semon in the Tyrol, quoted by Dr. Mackenzie, diphtheria can arise spontaneously. Not long since, in a secluded part of the country, in an isolated house, we were called to a case of diphtheria in a child, without any possible means of contagion that could be traced, and the affection ran through four children, to be followed in three out of the four with nasal signs and paralytic symptoms.

The strange power of inherent vitality which the poison of

diphtheria possesses is clinically shown and recognised by the manner in which it lies dormant in a house or locality for a long time previous to the occurrence of an outbreak. To this dormant period of inactivity it would seem as if there were no appreciable limit. It may be for months, or it may extend to years. This insidious lurking of the poison in the immediate neighbourhood of an outbreak, or its periodically recurring manifestations of activity, is shown in the case of New York, where diphtheria may be said (L. Smith) to be *endemic*. And indeed in this property of latent effusion and mystic contamination of the surrounding atmosphere, we find an explanation of the ready susceptibility with which, in all exanthematic states, the inflamed mucous membranes are attacked by the diphtheritic particles, and of the ready transition of simple catarrhal states of the throat, or of the mucous membranes elsewhere, into diphtheritic.* The practical lesson is obvious. A few years since we had under our care, during a short epidemic of diphtheria, a simple case of granular inflammation of the eyelids, which required gentle topical applications for its cure. Suddenly, after slight exposure, a most alarming attack of diphtheritic conjunctivitis supervened, which for days threatened the safety of the eye.

“The distance,” Dr. Mackenzie says, “at which the contagious principle can operate, as a rule, appears to be more limited than is the case

* In the British Medical Journal of January 4th, 1879, will be found the first part of the report of Dr. Eigenbrodt, embodying the opinion of himself and his colleagues on the causes of the intensity and extension of the epidemic in the Grand Ducal family at Hesse-Darmstadt. It would appear that the diphtheritic membranes were, from the first, of a peculiar type, having a discoloured (ecchymosed) appearance. The poison was transferred by kisses directly. But the important point is the previous predisposition to susceptibility, brought about by frequent attacks of catarrh of the mucous membrane of the pharynx and tonsillitis. Thus, in 1875, and again in 1876, Princess Victoria suffered from follicular tonsillitis; Princess Alice suffered from subacute follicular pharyngitis in the autumn of 1877; Princess Mary had some slight catarrhal attacks, with coryza occasionally, and one of these preceded the diphtheria; Princess Irene suffered from coryza and catarrh in 1876. Prince Ernest was a still more frequent sufferer, and had a constantly hypertrophied state of the tonsils. His Royal Highness the Grand Duke for many years suffered from a chronic catarrhal state of the pharynx, &c. Princess Ella (the only member of the family who escaped the epidemic) had enlargement of the lymphatics of the neck, and subsequent catarrh of the pharynx. Her Royal Highness had not for years suffered from any pharyngeal affection, and her illness commenced with a diphtheritic exudation on the right tonsil, thirty days after Princess Victoria was attacked, and twenty-two days after the beginning of the last case of disease in the family.

Dr. Thorne Thorne has, in the past year, strikingly proved, in the Local Government inquiries, the relationship which diphtheria maintains to simple sore throat.

in typhus or smallpox. Thus I have known an instance in which seven children were affected in a house which had a residence on each side of it, and a third opposite at a distance of only twenty-four feet. Although in all these buildings there were young children, no other case of diphtheria occurred. Other similar illustrations of this fact are on record. Under certain circumstances, however, the diffusive powers are increased, and, as appears to be the case in epidemics of influenza, the poison may be wafted over extensive tracts of country."

In fact, so numerous are the channels by which the poison may enter the system, that it would appear as if diphtheria in this respect almost exceeded, in its contagious qualities, other morbid poisons of a zymotic character. These instances narrated by Dr. Mackenzie are striking:—

"The remarkable case related by Dr. Paterson has an important bearing on the question of inoculation with diphtheritic membrane:—A man put his finger down the throat of a child suffering from diphtheria. The finger had a wound upon it at the time, which shortly after became ulcerated. All the constitutional symptoms of diphtheria subsequently appeared, and were followed by general paralysis of the extremities."

The spread of the poison by means of milk was last year proved by Mr. W. H. Power.

"In illustration of the first-named fact, the following case, which came under my own observation, may be cited:—A girl, aged six, who had been absent from home for five weeks, returned one afternoon at four o'clock. Her young brother, aged four, had shown symptoms of sore throat the same morning, but no suspicion was entertained that the disease was diphtheritic. These two children remained together till bedtime, but did not sleep in the same room. The next morning both of them had marked diphtheria, with an abundance of false membrane. The little girl had not been subjected to any infection before reaching her home. On the other hand, I have known one instance in which the disease occurred fifteen days after exposure to contagion:—A young lady, aged eighteen, insisted, contrary to the advice of her friends, in paying a visit to her cousins living in London, who were convalescent from diphtheria. She spent about two hours in their society, and then returned to her home in the country. Fifteen days after her visit she was attacked with diphtheria."

"Next in importance to age as a predisposing cause would seem to come *family susceptibility*. The liability of diphtheria to attack the members of certain families is well proved. Sir William Jenner lays

great stress upon family constitution as being ‘one of the most important elements favouring the development of the disease and determining its progress.’”

“Some remarkable instances of family susceptibility have come under my own notice. In one case a poor woman had three children of her own, and took care of two others in no way related to herself; her own children were attacked by the disease, and one of them died. The other two children—not her own, who were constantly in the same room with the little patients, never suffered from the disease. In another case four families occupied a house near Woodford, in Essex. In all of them there were several children. Two of the families were related, the mothers being sisters. All the children who were related to each other had diphtheria severely, whilst the children of the other two families escaped entirely. During the progress of the disease no attempt at isolation was made, the healthy children frequently entering the rooms of the patients.”

With regard to the protective influence of an attack of diphtheria, there is no question that it does afford, as in other affections of a kindred nature, protection from future attacks. But this rule has its exceptions.

“That the disease does sometimes recur I am well aware, for I have myself known three instances in which children have died from the second attack.”

“In my own case, I saw a child aged four with pharyngeal diphtheria in May, 1874, who died of laryngeal diphtheria under my care in July, 1875. I have seen the disease occur, in a mild form, three times in the same individual, at intervals of five months, a year, and two years.”

As regards the symptoms of the disease there is little new to offer. Classifying the disease according to its constitutional forms, Dr. Mackenzie thus divides it:—(1) *The typical form*; (2) *the mild, or catarrhal, form*; (3) *the inflammatory form*; (4) *the malignant form*; (5) *the gangrenous form*; (6) *the chronic form.*”

As to *site*, he is satisfied with the division into nasal diphtheria and laryngeal diphtheria, or croup.

The evening elevation of temperature, and the fall, as noticed by Faralli, on the fourth to the fifth day, and the presence of albumen in the urine, as well as the relation existing between these two symptoms and the development and progress of the disease, and the formation of the false membrane, are specially dwelt on—the albuminuria denoting, it should always be remembered, the danger of grave changes occurring in the kidney at the time, and, as a

consequence, a variety of brain symptoms, or, it may be, convulsions.

But we would direct special attention, in the chapter on Symptoms, to the remarks appertaining to what is rather ambiguously, as we conceive, termed "*catarrhal diphtheria*." The danger of our overlooking the simplest case of diphtheria is manifest. It is alike serious for the friends of the patient and the reputation of the practitioner. These milder forms of diphtheritic inflammation are those which most frequently perplex the physician by the obstinacy of the sequelæ. The description is so characteristic that the perusal, as detailed by Dr. Mackenzie, will amply repay any practitioner who will give to this chapter on "Symptoms" a critical study. The slight constitutional disturbance, the mildness of the local trouble, the supervention suddenly of the paralysis, are the most marked features of these frequently occurring cases. But in the chapter on Diagnosis the most likely sources of error, on the part of the practitioner, are carefully defined. It is better in these cases to err on the side of too great caution than on that of careless apathy. Nor is it requisite to alarm, or create unnecessary fear, by speaking of a "diphtheritic throat," or of a given case as one of diphtheria, until we are justified in cautioning friends, in order to conscientiously discharge our duty by a close watch on the progress and symptoms of any particular case of "*sore throat*."

Dr. Mackenzie devotes special chapters to the consideration of "Paralysis," "Diagnosis," and "Prognosis." We might have wished that the two latter points were more fully entered on—in fact, this is, we conceive, the only weak point in this admirable treatise. But, though the chapter on prognosis is short, the directions are concise and to the point, as, for instance, in this paragraph:—

"It must also be borne in mind that in certain families diphtheria has an exceptional tendency towards a fatal result. With regard to the *special* symptoms on which to found a prognosis, the following considerations chiefly deserve attention:—High temperature, extreme prostration, hæmorrhages, or urgent vomiting at the commencement of an attack, are signs indicative of extensive general infection, and must, therefore, be looked upon as of very serious prognostic import. Valuable information may be gained from the character and extent of the false membrane. *Ceteris paribus*, the prognosis is serious in proportion to the thickness and extent of the exudation."

Reading, as we did, anxiously, the chapter on Treatment, we

can candidly say that, as this is the portion of any new work on this subject which will be most eagerly scanned by a practitioner, so are the various plans of treatment done full justice to by Dr. Mackenzie. Every matter of detail in relation to the general support of the patient—the advisability of depletion, recuperative agents, such as iron and quinine; specific remedies, as mercury, sulphide of potassium, bromine, and balsams; the various antiseptics, notably the iron salts, salicylic acid, carbolic acid—is noticed. So, also, is the local treatment.

For our own part, we cannot say that, once the diphtheritic membrane has been formed to any extent on the fauces or throat, much benefit has been derived from any of the vaunted specifics—and all have been tried. Of all such we prefer the chlorate of potash with boracic acid in glycerine, alternately with perchloride of iron (3ss. ad. ʒi.) dissolved in glycerine, or the lactic acid; but often we are forced to try, in many cases, a new remedy, having failed with those we placed most reliance on; and thus permanganate of potash, chlorinated soda, syrup of chloral, sulphur, &c., &c.—all have their advocates. Dr. Mackenzie advises, as varnishes, the gums, benzoin, tolu, mastic dissolved in rectified spirit or in ether (1 in 5); and, the membrane having been dried with blotting-paper, a coating of the varnish is applied.

Remarks on the use of the introduction of steam, the value of the “portable croup tent,” and the general precautionary measures to be adopted in a suspicious case, as also the prophylactic rules, complete a chapter replete with information. The remarks on tracheotomy and the indications for the operation, as also the statistics, are reserved for the chapter on Laryngo-tracheal Diphtheria, or Croup.

On the 22nd of October the Report of the scientific committee appointed to examine into the relation existing between the diseases commonly known, respectively, as Membranous Croup and Diphtheria, was presented to the Royal Medical and Chirurgical Society. The complete Report of the committee has not yet been given to the profession, nor has the Report been fully discussed. The practical result of the labours of the committee might be summarised as follows:—

1. The causes of membranous inflammation of either larynx or pharynx are various, and may be briefly classified as those of Diphtheritic Infection, Zymotic, and accompanying the Exanthemata, Accidental or Traumatic, Catarrhal.

2. Membranous laryngitis may pass into or impart the pharyngeal affection.

3. It is not practicable to show an absolute line of demarcation between the laryngeal and tracheal inflammations, either from the nature of the causes, the presence of albuminuria, or the anatomical lesion present.

4. The only marked division observed being between membranous and non-membranous laryngitis, the distinction hitherto drawn by many between Croup and Diphtheria cannot be preserved, and the committee propose to use the term “ ‘ Croup ’ as a clinical definition implying laryngeal obstruction occurring with febrile symptoms in children,” while they regard “ ‘ Diphtheria ’ as the anatomical definition of a zymotic disease which may or may not be attended with croup.”

Dr. Mackenzie devotes Chapter IX. of the work to Laryngo-tracheal Diphtheria or *Croup*. Tracing the first use of the word “croops,” or “croup,” to Dr. Patrick Blair, in 1713, and subsequently to Home, he gives the synonyms which express this peculiar breathing or vocal sound in Dutch, *Geroop*; Icelandic, *Hrøpa*; Anglo-Saxon, *Hreopan*; Gothic, *Hropjan*; Old German, *Hrof*; Modern German, *Ruf*; or “it may be derived from the Gaelic, *Crup*, signifying a *contraction*—*i. e.*, a contraction of the throat. Tracing the various views which have prevailed from time to time on the distinct nature or the identity of the two diseases, the author declares his conviction that “the pathological differentiation of the phenomena must be abandoned.” This proposition has been amply substantiated in all the cases we have seen, in which the naked-eye appearances after death have been observed, nor during life has any practical difference been detected when the membrane has been fully developed. The microscopical observations of Wagner and Rindfleisch confirm this opinion. Yet we can hardly feel justified in asserting that clinically the two diseases are one and the same. Broad as our views may be, and clearly as we may recognise the fact that we cannot define an *anatomical* distinction, still, beyond question, there is sufficient of difference in the localisation of the disease, in the glandular complications, in the frequent renal mischief, in the adynamic fever, in the characteristic sequelæ, and the dreaded epidemic type of the poison, and its contagious nature, to warrant a *clinical* differentiation. Nor, indeed, would we consider it wise that this clinical contrast should be forgotten by the practitioner. That there are forms of membranous croup, or,

if we consider the term more accurate, laryngo-tracheal diphtheria, which require a method of treatment widely different from the one we have to follow in typical diphtheria—which, commencing in the pharynx, gradually involves the larynx—is unquestionable. Nor can we quite agree with Dr. Mackenzie that “cases of sthenic croup are rare.” We have seen many such in which the prompt use of the lancet saved life; and when we find an advocate for the complete identity of the two diseases forced to devote a separate chapter to the “etiology,” “symptoms,” “diagnosis,” “pathology,” “prognosis,” and “treatment” of a so-called variety of the affection he is discussing, we may safely conclude that the special form has sufficient distinctive features to warrant our regarding it in a special light, and as anatomically and clinically differing. Most important is the caution during the first stage of the disease to examine the sputa.

“Children very often do not expectorate at all, but anything that is brought up must be put into a glass vessel and gently shaken with a little pure water. The mucus dissolves, and flocculi or small shreds of false membrane, if present, become visible.”

Thoroughly do we agree with the statement that, though a certain risk of the loosened membrane obstructing the larynx has to be run in the case of emetics—

“This risk must be incurred, though valuable time should never be wasted on the use of emetics, when the only alternative is the performance of tracheotomy. Tickling the fauces will occasionally be sufficient to excite the desired action, but as a rule it is necessary to resort to drugs. Cardiac depression is so common an accompaniment of diphtheria that it is unwise to employ any emetic by which it is likely to be increased. Tartar emetic must, therefore, be especially avoided.”

Sufficient stress is hardly laid on the early use of the emetic, ipecacuanha, and the great danger of the late employment of such a depressant. The early and free use of hippo, the judicious and timely employment of the warm bath, the securing of a proper temperature, and the administration of small doses of calomel, though old remedies, have nevertheless saved many lives in croup. We notice that nothing is said of the use of calomel in croup. As to the value of the steam atomiser, we can thoroughly endorse all that is said in its favour. Dr. Mackenzie advises ice to the neck, in the first stage of the disease, in an ice-bag. This plan, however,

we have of late relinquished for the simple warm dressing of lint with protective; and we can testify to the benefit which we have often seen follow in the earlier stages of croup from the light touching of the skin over the first few rings of the trachea with the liq. epispasticus, the mild blister thus produced being covered with the warm dressing. Dr. Mackenzie uses a squirrel-tail brush, with the hairs on the laryngeal portion of the brush directed upwards to remove any false membrane loose in the larynx—the common laryngeal brush and insufflator we employ for the application of solutions or powders. We would have desired greater detail in the portion of the work devoted to Tracheotomy and the methods of operating, as also the author's own statistics and personal experience of results. Of 5,922 cases occurring in the practice of Trousseau, the Hôpital des Enfants Malades, Hôpital Sainte Eugénie, the Hospital for Sick Children, Professor Langenbeck, Dr. Solis Cohen, there have been 1,542 absolute cures.

Unfortunately we cannot ourselves speak favourably of the ultimate results of the operation. In five cases of pure diphtheria, in two of which the operation was performed as a *dernier ressort* in the later stages, and in three when apnoea was almost approaching—though the first recovery was marvellous in all, lasting in four out of the five for from two to three days, the patients ultimately succumbed to pneumonia and extension of the membrane. We think it of extreme importance to draw attention to the caution given by Dr. Mackenzie, “immediately after the operation to draw out any loose false membrane either with the croup brush or with an aspirator accurately applied to the mouth of the cannula.” In the paper by Mr. Parker, read before the Royal Medical and Chirurgical Society in November last, the same step is strongly urged, whether it be membrane or mucus, and he enunciated the following dictum:—“The presence of membrane in the trachea in a fatal case of membranous laryngitis after tracheotomy, must be regarded as evidence of the want of due care on the part of the surgeon in charge, just as much as would the presence of a piece of gut in the inguinal canal after herniotomy, or a calculus in the bladder after the operation of lithotomy.” In seventeen cases of tracheotomy in membranous laryngitis at the Great Ormond-street Hospital, there had been eight deaths—an unusually successful result.

We congratulate Dr. Mackenzie on this addition to the literature of diphtheria. The work is full of practical hints, and being written

in his well-known lucid style, can be perused by the busy physician with pleasure. It is a book which should find its way into the hands of every practitioner.

H. MACNAUGHTON JONES.

On the Photo-chemistry of the Retina and on Visual Purple. Translated from the German of Dr. W. KÜHNE, Professor of Physiology in the University of Heidelberg. Edited, with Notes, by MICHAEL FOSTER, M.D., F.R.S.; Fellow and Prælector, Trinity College, Cambridge. London: Macmillan & Co. 1878.

THIS translation of Kühne's valuable monograph has been made, as Dr. Foster informs us in the preface, by Mrs. Foster, and we can well believe she "found the task of converting Prof. Kühne's somewhat idiomatic German into readable English not free from difficulty." Dr. Foster himself went carefully over the whole of it, and he has added some valuable notes to it in the form of an appendix.

The whole subject is one so full of interest, and the investigations of Prof. Kühne have added so much to our previous knowledge of it, that we venture to think a brief epitome of these researches may not prove unacceptable.

Kühne has, it seems, been reproached by some for having made use of Boll's well-known discovery of the sensitive purple colour of the retina, but, as Dr. Foster points out in a note to his preface—

"To Boll must undoubtedly be given the credit of the discovery that the retinæ of many animals possess a sensitive purple colour (for the mention of it by previous observers does not amount to a discovery), and he therefore will rightly share in all the fame belonging to the subsequent development of that discovery. At the same time the study of Boll's writings can leave no doubt on the mind of the candid reader that at first Boll did not realise that the visual purple undergoes changes through the action of light after the death of the animal; he attributed the bleaching after death to *post mortem* decomposition. It was Kühne who was the first to observe that light bleaches the visual purple after death."

Prof. Kühne has bestowed great pains on his researches, and, though in the main agreeing with most of Boll's statements, he, nevertheless, in some important respects, differs from his conclusions. One of the first points to which he directs attention is, that the haste in the removal of the eye and the taking out of the retina,

which Boll considered so essential for the exhibition of the visual purple, is after all not at all necessary. He states that the retina may be quite leisurely spread out under the light of a good gas-lamp, and yet the colour will last from twenty to thirty minutes.

He made experiments with various reagents for the purpose of determining their effect upon the colouring and sensibility to light of the retina. He found that the colouring was destroyed by heating to 100° C., by alcohol, by glacial acetic acid, and by both concentrated and 10 per cent. solutions of soda. Various other reagents, on the contrary, had no effect—*e. g.*, a 0·5 per cent. solution of chloride of sodium, solutions of alum and of acetate of lead, 2 per cent. solution of tannic acid, strong ammonia, &c. Indeed the action of the latter appeared to be to intensify the colour. Once the colour was thoroughly bleached it never returned. In the case of the pig he found that it is unnecessary to keep it in the dark immediately previous to the removal of the retina, and he accounts for Boll's failure in demonstrating the colour, on one occasion, to exposure of the retina to light during the operation of removal, for, as we shall see further on, he proves by various experiments (p. 7) that so long as the retina remains in the eye, lying upon the choroid, the colour is restored. Long-continued exposure to direct sunlight can, and does, nevertheless, destroy it. It may legitimately be conceived—and, indeed, Boll expresses some such opinion—that the visual purple in the living eye is being constantly destroyed and as constantly renewed. The experiments which Kühne undertook with the view of elucidating this problem are very beautiful and ingenious. "The oculist," he says, "led by experience would immediately seek for the process of regeneration in the nourishment brought by the circulating blood; for this is a favourite way of accounting for most of these kinds of events." This explanation, however, at once falls to the ground, "since an eye, when taken out and opened, exhibits the same apparent indifference to light as when connected with the whole body and the nutritive currents." An idea immediately suggests itself that the pigment acts as a species of screen, preventing the greater intensity of light which would naturally be present had the retina a white background to rest on. Removing the retina and spreading it out upon a black surface, with the rods downwards, was not, however, found to have much effect upon the time of bleaching. The following experiments seem to prove almost exclusively that it is the choroid, including the retinal epithelium, which alone possesses the property of protecting

the purple from bleaching in light. The first experiment consists in removing the retina in such a manner that shreds of choroid still remain adherent to it. This can be accomplished by cutting out the bulb so as to leave a hole at the entrance of the optic nerve. By this means the spot which offers the chief resistance to the removal of the membrane is got rid of. The retina is now spread out smoothly, and exposed to the light until bleached. If the dark strips of choroid are now removed, the parts lying under them will be found deeply coloured.

Again, by slightly tearing the membrane in a bisected bulb, so as to make some folds in it, then allowing the light to shine upon it, and afterwards quickly pulling out the whole retina, it will be seen that where the folds were there are white stripes, whereas the rest remains red.

Another very interesting experiment consisted in raising up a considerable portion of the retina and inserting under it a piece of porcelain, then exposing the whole to light until bleaching took place, after which the part was allowed to sink slowly back on to the choroid, where it remained for some minutes. On now removing the entire retina it was found to be uniformly red all over, nor could any line be detected marking off the portion which had been raised. This proves that the retina may recover its purple colour again by simple contact with its natural support. Kühne has gone even a step further, and cut out a flap of the retina, bleached it on a plate, and then laid it back again upon the exposed pigment. This also regained its colour. So frequently, indeed, did this succeed that he was led to try whether pieces of tissue paper, inserted in the same way, would not also become coloured, thinking it might be due to some red secretion. They came out, however, quite colourless.

In the closing paragraph of Part I. Professor Kühne throws out the suggestion that the portion of the choroid which serves to regenerate the purple "is to be sought not so much in the dermic layer of the choroid as in the epithelium in which the rods are embedded, and which has been rightly considered as part of the retina. 'The retina,' he continues, "so long as it is maintained in its natural connexions with this epithelium, resembles not so much a photographic plate as a whole photographic workshop, in which the operator, by bringing new sensitive material, is always renewing the plates, and at the same time washing out the old image."

The second part of the monograph opens with an account of the literature of the subject, which shows that "the red colour of the retina had been seen by many observers long before it had obtained so great an interest through Boll's communications." Krohn and Hensen, in 1839 and 1842, both recognised it in the retina of the Cephalopoda. In 1851 and 1857 Heinrich Müller and Leydig both spoke of its occurrence in the frog; and in 1866 the matter was again mentioned by Max Schultze.

Kühne evidently does not participate in the rather premature expectation of many who seem to imagine that from the recent discoveries of the existence and behaviour of this visual purple "we are now in a position to understand tolerably well how the excitation of the optic nerve is brought about by the action of light." For while admitting "that the various decomposition products of the visual purple—namely, the orange, the yellow, and in particular the colourless substances, serve as chemical stimuli for the ends of the optic nerve, while the original visual purple serves as an inert medium having no effect upon them," he nevertheless adds the caution "that the visual purple must not be looked upon as the only substance in the retina which is sensitive to light."

For many years the transformation of the movements of the luminous ether into chemical processes has had considerable currency, but there was nothing to justify the assumption that any of the substances necessary for such action could be recognised by their colour—that one of them has been so Kühne regards as very fortunate.

Proof is still wanting that all visual organs are furnished with this purple, or that when it is bleached, blindness ensues. Closely connected with the physiological part of the question we find an anatomical one of considerable interest and importance—namely, the mode of termination of the optic nerve-fibres. If it is supposed that the terminal fibres "embrace the refractive bodies in the inner limbs of the rods and spread out in the red outer limb, in that case the final terminations—which might continue to be considered similar to the fibres of the optic trunk and indeed to all genuine nerves in respect to structure, composition, and irritability—would be in the most simple manner stimulated, through the medium in which they lie becoming loaded by photo-chemical action with some caustic agent." If this be so it might be reasonable to expect that if the posterior surface of the retina were applied to an exten-

sive section of a motor nerve of a frog, and then illuminated, the stimulus would be conveyed to the latter. Such an experiment Kühne states he has frequently tried, but hitherto with negative results. He says, however, that he does not consider the hypothesis so disproved, as many and obvious objections may be raised against the experiment.

Another hypothesis which he considers it right to mention—namely, that the visual purple may be the result, and not the cause, of an irritation of specific nervous elements by light, is clearly contradicted by the sensitiveness to light of all dead retinas. Before, however, being aware of this, Kühne had not omitted testing fresh retinas of frogs in the dark with every kind of electric stimulus, but always with negative results.

To find out whether all the other sensitive elements of the retina contained this purple was the next task Kühne set himself to investigate. He had already been struck by the fact that he had never seen any trace of it in the cones of the frog, and further examination failed to discover it there. The evidence of its existence in the retina of some birds (pigeons and fowls) which contain a large number of cones also proved fruitless; and this, it should be noted, is contrary to the experience of Boll, who maintains that it is present in the retina of pigeons.

Kühne examined the eyes of two owls of different species; in the one he found the purple colour spread uniformly over the centre of the posterior of the retina, and in the other it was less so. In a tower falcon he found the colour in stripes and spots, and confined to those places where there were few cones. In the bat the retina was colourless. In the badger the colour was present, as also in the eel and loach. In the eel the colour was more intense than in any other animal he examined, with the exception of the owl. The eel has no cones in its retina.

As regards the human eye, Kühne says he has only been able to examine one pair of eyes "which were in any way in a condition in which they could be of any use." Even in this case the examination was not made till two days after death. Special precautions, however, had been taken that light should be excluded. About half a minute before death light was excluded, and a dark covering was afterwards laid over the head and eyes of the corpse. As far as could be judged without absolute certainty, on account of the number of hours which elapsed after death, the examination seemed to show that the portions of the retina which were rich in cones

possessed very little purple, and the parts consisting entirely of them—viz., the macula lutea and fovea—had none. In the carefully prepared Appendix which Dr. Foster has added to this translation there is a note from which we learn that Kühne has since had opportunities of extending and corroborating the above statements. “He finds that the purple is present in the rods only, being entirely absent from the cones.”

The examination of the eyes of apes was confirmatory of these observations; and the eyes of snakes, in which the rods are absent, showed no purple colour, nor could any be detected in the eyes of lizards. This had also been noticed by Boll.

In the retina of the carp the difference between the purple rods and the uncoloured cones was very marked.

It is a point worthy of note that Kühne found the retina of a foetal calf quite clearly coloured purple—“the colour, when exposed to light, first passed into yellow, and then entirely disappeared; in this case the rods were clearly recognisable under the microscope as delicate short palisades.”

The next question which suggested itself to our author was the possibility of isolating the purple colour. This, after numerous experiments, he succeeded in accomplishing by means of a solution of bile. For the details of the method we must refer the reader, who is anxious to investigate the matter more deeply, to the work itself. Having obtained the colour in an isolated condition, Kühne then proceeded to test its capacity of absorption of light of different wave-lengths. He speaks of the failure which had attended all his previous efforts “to become acquainted with the absorption spectrum of the retina, by holding the glass-plate on which it had been spread out before the slit of the spectrum apparatus; either the absorption was too weak or there were so many horizontal shadows and stripes in the image that nothing could be made out clearly.” We can bear testimony to the same difficulties, having experienced them when trying some similar experiments about six years ago with the assistance of Mr. C. F. Burton.*

The results Kühne arrived at are briefly as follows:—He found “the absorption begins in the yellow, being very weak in the yellow of the D line, increases to E, the rise being very rapid at the beginning of the green, and a further increase at the junction of the green-blue and blue, and decreases again towards the violet.”

His next research was the determination of the absorption power

* Late Assistant at the Astronomical Observatory, Dunsink.

of the visual purple in its natural position in the retina, by means of the objective spectrum—for by that he hoped to get rid of “the dispersion in the retinal membrane which interfered with all experiments in which the retina was placed between the source of light and the prism”—and this hope was realised as soon as he “placed the frog’s retina, laid on a slip of milky glass, in the spectrum of a Drummond light.” He sums up the action of monochromatic light on visual purple as follows:—

“(1.) Monochromatic light decolourises and bleaches visual purple as white light does, only considerably more slowly, corresponding to its lesser intensity.

“(2.) Of all kinds of monochromatic lights the following act with decreasing rapidity:—greenish-yellow, yellowish-green, green, bluish-green, greenish-blue, cyanic blue, indigo blue, violet, later pure yellow and orange, still later ultra-violet and red. The extreme red and ultra-violet rays are not entirely without action, but the commencement of the ultra-violet is more active than that of the visible red.

“(3.) The transitional stages of visual purple as it passes to white—namely, the products of bleaching—orange, chamois, and pale yellow—are least resistant to monochromatic light in indigo and violet, are more so at the beginning of ultra-violet than in the range from cyanic blue to orange, and are more resistant in pure red.”

Kühne’s next experiments were on impure coloured lights. Coloured glass he found did not answer for these experiments, “owing to the small choice they offered and to the difficulty of obtaining intermediate tints.” By means of solutions of carminic acid and picric acid, cupric oxide, and cupric sulphate and picric acid, he was able to get a very good red-green and blue-violet. The following is the result of the bleaching action of the retina he obtained in these experiments:—In a blue, which seemed to his eye so deep that he was unable to manipulate in it much better than in the dark, the purple bleached in two to three hours; while in the green, which appeared very luminous, it took from three to four hours. In a brilliant red “the action first became distinct after the lapse of sixteen hours, and it was not till twenty-four hours that all traces of colour vanished from the retina.”

Experiments on the purple of the living eye were those which next engaged Kühne’s attention, and in recounting them he again reasserts both the statements he previously made with regard to the influence of the choroidal epithelium in restoring the purple colour in a removed and bleached retina, when the latter is

reapplied to its surface. The visual purple during life, however, is very durable, and the prolonged exposure which is necessary in order to destroy it, would seem also to have the effect of removing this regenerative property from the epithelium. This appears to be placed beyond a doubt by the following experiments:—

“The retina is removed carefully from the eye and exposed until it has become bleached. The fundus of the eye is now exposed to intense light for 20 to 30 minutes, the retina meanwhile being put back into the dark. If the retina is now carefully replaced upon its old support, the red colour will scarcely, or not at all, return, even though the rods be allowed to remain for hours in contact with the epithelium.”

That this is not due to decomposition or death of the epithelium, but simply to the action of the light, is proved by the fact that—

“An equally long exposure of the uncovered epithelium in the dark scarcely at all affects its power of regeneration, as may be shown by the positive result of an experiment the reverse of the above, in which the retina only is exposed to daylight.”

The experiments on the living retina with coloured lights, though on the whole confirmatory of those made on the removed retina, did not satisfy our author, and he says:—

“After some positive results in the midst of a very large number of negative ones, I was about to give up the whole arrangement, had I not accidentally made the following observation. One of the frogs placed in the blue light had kept his eye steadily fixed on the flame, and I found in the retina, although it had been exposed to the light for only fourteen hours, a most beautiful image of the gaslight standing out perfectly colourless on the deep red ground of the bacillary mosaic. The retina came out of the eye wonderfully smooth, without any merit due to me, and lay spread out completely flat on the cover-glass, every rod standing upright with its pigment-face end uppermost.”

He goes on to say:—

“This suggested a simple means of obtaining at pleasure photographs on the retina. It is only necessary to render the frog motionless with urari, and having cut away the nictitating membrane, and having caused the eyes to project somewhat, by stuffing a ball of paper into the mouth, to expose the animal for about two hours before a flame. The best distance is about 35 to 40 cms.”

Kühne, in conclusion, gives a most interesting account of his experiments and method of obtaining what he terms optograms, or

photographs upon the retina. After numerous failures, he at length, partly by accident, succeeded in obtaining very beautiful optograms. His method, as perfected, may be briefly described as follows:—The eye is opened and thoroughly freed from the vitreous humour, and immediately after receiving the image it is placed in a solution (4 per cent.) of alum, which has the effect of hardening the retina. In this it remains, in the dark, for twenty-four hours. It is then taken out and placed upon a leaden support, under water. Upon this leaden support the optic entrance is punched out, and the membrane is then seized at a point above the optic disc. “If the operation is carried out without a mistake, the retina can often, with a slight jerk, be pulled out of the solid sclerotic cup like a delicate shell, without its collapsing or creasing at all.” A small porcelain capsule, hardly bigger than half a rabbit’s eye, is now brought, upside down, under the water, and upon this the retina is allowed to sink slowly. By means of a suitably-bent strip of lead, which has been slipped under the capsule, the latter can be raised into a horizontal position.

Kühne has also found a method of fixing these optograms by drying the retina in a sulphuric acid desiccator.

In an epitome such as this, several subjects of the greatest interest and various important details have necessarily been passed over. It is chiefly intended for those who have not the leisure or opportunity of consulting the original. To those who take a deeper interest in the matter, we most cordially recommend this valuable monograph. The subject is one of absorbing interest, and though it may never add much to ophthalmology in general, it will doubtlessly enrich that branch of it which relates to physiological optics.

C. E. FITZGERALD.

The Sight and how to Preserve it. By HENRY C. ANGELL, M.D.;
Professor of Ophthalmology at Boston University. Pp. 63.
London: Hardwicke & Bogue. 1878.

WE wish we could believe that the publication of popular works such as this really does any good. The object, no doubt, is excellent, yet it is impossible to shut our eyes to a danger which it seems must inevitably follow the widespread distribution of such books; and it is this—that the laity, relying upon the general principles enunciated in these treatises, will consider themselves competent to

prescribe for the disorders which they and their friends suffer from, and to play the part of amateur ophthalmologists. The number of people who *dabble* in scientific subjects is now-a-days greatly on the increase, and at no time was it more urgently needed to proclaim aloud that "a little learning is a dangerous thing" than in this age of primers and concentrated sketch-books on matters relating to science, literature, and art.

It should not be forgotten that to acquire a proficiency in the science of ophthalmology demands a special training, and that the particular branch of it which relates to the anomalies of the accommodation and refraction is one involving some of the most difficult problems that the specialist is called upon to solve. Consequently, books purporting to treat such matters in a popular manner should not be lightly taken in hand. Above all things, if we must have these books, it is of the utmost importance that they should be extremely accurate in every statement they contain, especially when it comes to such a question as the treatment of a delicate organ like the eye.

Though, in the main, we have no cause to complain of the very popular and lucid description Dr. Angell gives of the eye and its anomalies, there are, nevertheless, one or two points to which we would take exception. In speaking of the use of eye-glasses in weak sight (asthenopia), after very properly advising those who suffer from this affection to consult a competent oculist, he adds (p. 17):—

"For the benefit of such as are unable to get proper advice, it may be as well to say that the convex glasses will probably require to be about 48-inch focus, and that they are to be worn only in reading, sewing, and such occupations as require the accommodative apparatus of the eye to be brought into use."

Of course he is here merely speaking of cases of accommodative and muscular asthenopia not depending on any anomaly of refraction (myopia, hypermetropia, or astigmatism); but, as he subsequently points out, weak sight, or asthenopia, is of very frequent occurrence in myopia (p. 30). We need scarcely say that the use of convex glasses in such cases would be highly injurious; yet we can quite conceive a patient so affected, and relying on the advice given above, persevering in their use, because Dr. Angell says (and very truly, be it noted) that "to accustom the eyes to the help of glasses may require some days or weeks and considerable patience."

Dr. Angell may say, "There is no danger of such a mistake,

because at p. 31 I give the methods of testing for near-sight (myopia)." But we answer that the tests he gives are valueless unless controlled by an ophthalmoscopic examination. The fact that test-type "No. 20 cannot be seen at twenty feet, nor No. 50 at fifty feet," is no positive proof of the existence of myopia, as the same may occur when hypermetropia is present. Nor is it a "more certain mode of detecting near-sight to put a pair of weak concave glasses before the eyes," for it is not at all uncommon to find the same proceeding in hypermetropia frequently "improves the vision for distance" to a most remarkable extent. It is easy to see to what serious complications advice like this may lead; and though, as we said at first, in the main agreeing with most of what the author states—and, indeed, highly commending some admirably practical remarks of his on progressive myopia in children—we are, nevertheless, totally opposed to the idea of suggesting that people, under any circumstances, should, on general principles, undertake for themselves such an important and delicate task as the selection of suitable spectacles.

Notes on Physiology. By HENRY ASHBY, M.B.; formerly Demonstrator of Physiology, Liverpool School of Medicine. Pp. 238. London: Longmans, Green, & Co.

THE notes are compiled for the use of students preparing for examination, and, consequently, they are not free from the imputation of being really a final "cram." To give a bird's-eye view of such a subject as physiology is a difficult, if not an impossible, undertaking, and Dr. Ashby's condensation, if not a success, is at least not inferior to any of the same limited size. The opening chapter, on Physiological Chemistry, is meagre—Peptones, for instance, being dismissed in a single sentence. The histological notes are fuller than the physiological, and the chapters on the Nervous System are as complete as the space at the author's command will admit of. Dr. Ashby has certainly epitomised a great number of facts into his 238 small pages; but we fear that note-books of this kind will lead the careless student to trust to his recollection of details, rather than to follow out the processes by which the details are reached.

THE BOSTON
SOCIETY FOR
MEDICAL
OBSERVATION

PART III.

HALF-YEARLY REPORTS.

REPORT ON PUBLIC HEALTH.*

By CHARLES A. CAMERON, M.D., L.K. & Q.C.P.I., Diplomate in Sanitary Science, Cambridge University; Fellow and Professor of Chemistry and Hygiene, Royal College of Surgeons in Ireland; Medical Officer of Health and Analyst for Dublin, &c., &c.

ETIOLOGY OF DIPHTHERIA.

THE sad death of the Grand Duchess of Hesse-Darmstadt from diphtheria has invested this disease with a peculiar and melancholy interest at the present time. Known to exist for many centuries, it only occasionally assumes an epidemic form, and for long periods of time *apparently* disappears. Since 1818 it has, with some degree of regularity, appeared epidemically in various parts of France. It is supposed that the severe outbreak of sore throat in England in 1748, which is recorded by Dr. Fothergill, included cases of diphtheria, if that disease was not the sole cause of the epidemic. In 1855, after the lapse of more than a century of almost complete immunity, diphtheria invaded England, and spread from thence to Ireland, and during the following five years it appeared in various parts of the United Kingdom. The Medical Department of the Privy Council commissioned, in 1859,^b Dr. (now Sir William) Gull, Dr. Greenhow, and Dr. Burdon Sanderson to investigate the nature of the disease. It was proved to be communicable, and it was considered that the virus of the disease was capable of existing outside the human body, though in general it was much more frequently propagated from individual to individual beneath the same roof, than from household to household. The poison was found to be persistent in houses once infected. The virus of the disease was

* The author of this Report will be glad to receive any books, pamphlets, or paper, relating to hygiene, dietetics, &c. They may be forwarded through the agencies of this Journal.

^b Second Report of the Medical Officer of the Privy Council for 1859.

pronounced not to be of spontaneous origin, but to resemble the *materies morbi* of smallpox, scarlatina, and diseases of that class. Diphtheria was found to prevail more in low-lying, damp situations, than in elevated and dry districts.

In 1866 Dr. Burdon Sanderson investigated an outbreak at Waltham Abbey. It was limited to a low-lying district with a highly polluted soil. The origin of the infection could not be detected.

In 1871 the disease appeared in an elevated district—Newton Valence—but the water supply of the place proved to be scanty and polluted, and there were other serious sanitary defects present.

Dr. Thorne Thorne, in 1872, investigated a very fatal outbreak at Andover. The origin of the disease was a mystery which was not solved. The water supply was bad, but so was the water supply of thousands of other places where there was no diphtheria. Several other outbreaks from 1872 to 1877 have been inquired into, but in no one instance could the infection be traced to an external source. One serious epidemic occurred in 1876–7 at Great Coggeshall, in Essex; the fatal cases numbered 170.

The deaths from diphtheria in England and Wales were 385 in 1855; 603 in 1856; 1,583 in 1857; 6,606 in 1858; 10,184 in 1859, since which years the deaths have varied in number from 2,152 in 1872, to 6,507 in 1863. The disease is most rife during the first and fourth quarters of the year. Its victims are most commonly children.

It appears to be generally proved that damp houses or soils, bad water, and ill-ventilated dwellings, spread the disease. Dr. Thursfield, however, states, on the authority of Dr. Hill, that diphtheria is most prevalent in the highest and driest districts of Birmingham; but this fact is probably due, according to Dr. Hill, to the presence of sewer gases in those localities.—(*Lancet*, August 10, 1878.)

Dr. Thursfield in his articles on diphtheria, from a preventive point of view, published in *The Lancet* last August, is of opinion that diphtheria prevails inversely to typhoid fever. He quotes statistics to prove that it is more common in counties than in towns—assuming in his calculations that the populations of the former have remained stationary since 1871, whilst allowing for increased (“corrected”) populations in the towns. Dr. Russell, Medical Officer of Health for Glasgow, has, however, since pointed out that there has been a large increase of population in most of the counties which Dr. Thursfield quoted. Still there would appear

to be a larger number of cases per 10,000 inhabitants in country districts as compared with the larger towns.

Last year there was a serious epidemic of diphtheria in Kilburn and St. John's Wood, London. In all, 302 cases occurred, of which 38 proved fatal. Mr. W. H. Power investigated this outbreak for the information of the Local Government Board, and came to the conclusion that it was caused by the use of milk infected by the poison of diphtheria. He found that nearly all the persons affected had been supplied with milk from a particular dairy. Mr. Power was not able to prove that the milk had been adulterated with water which had been contaminated with diphtheritic poison, or that even polluted water had been used to wash the pails, cans, &c. The persons connected with the dairy had not suffered from the disease. The milk from this dairy was supplied to 473 households, of which 68 were invaded by the disease; on the other hand, there were but 30 cases of the disease amongst the other households of the districts, 2,227 in number. Thus it was discovered that the proportion of households affected was 11 times greater in the case of the consumers of the milk of a particular dairy than of those supplied with milk from other dairies. A point against the milk is the fact that the persons chiefly affected were young children, who were the chief consumers of the milk.

Mr. Power acknowledges that he is unable to explain how the milk became affected with the virus of diphtheria, but suggests that the poison may have originated in the cows themselves. If this hypothesis be true, it follows that a cow under certain and as yet unknown morbid conditions may yield milk which is capable of, when drank, setting up in man the symptoms of diphtheria.

Schweniger, of Munich, has experimented on the inoculation and nature of the virus of diphtheria. He considers that the fungi found in the false membrane are not the bearers of contagion, because they are found only on the epithelial layer, and because they do not produce by inoculation any symptoms other than those caused by the introduction into the blood of the ordinary organisms which accompany putrefaction.

FALSIFICATIONS OF MILK.

A long list of alleged adulterants of milk is given in the books, some of which, there is reason to believe, are purely apocryphal. In 1862 I was appointed Analyst for the city of Dublin, in which city there have been convictions for selling adulterated milk

and other articles every year since, and including, 1863. I was appointed analyst for other places in Ireland under the old Act of 1860, repealed in 1872. I have therefore much experience as a public analyst, especially at a time when there were no prosecutions of food adulterators in England, or, as a rule, in Ireland. During this latter-mentioned period adulterators were not exposed to much danger of detection, and I believe their practices were conducted upon a very much larger scale than is the case at present, owing to the operations of the anti-adulteration Acts, even with all their imperfections of construction and administration. I mention these matters for the purpose of showing that I have had excellent opportunities for discovering all the adulterations of milk that have lately been practised in Ireland, and the following notes are a brief record of this experience:—

Adulteration with Water.—Water is, with but very rare exceptions, the only adulterant of milk used in Ireland. It is sometimes liberally used. Many persons have been convicted in Dublin for having sold milk adulterated with from 50 to 120 per cent. of water—that is, to the 100 parts of pure milk, 50 parts to 120 parts of water were added. In my certificates I state the amount of water added *to*, and not the amount of added water *in*, the milk. The most extreme case of adulteration with water which I have met with occurred some years ago at the Workhouse, Mountmellick. The doctor noticed that an undue proportion of the children were suffering from rickets, and showing signs of under-nutrition. Acting on his suggestion, the guardians sent me three samples of the milk supplied by three different contractors; one I found to be a mixture of two parts of milk and one of water, the second was composed of equal parts of milk and water, and the third consisted of two parts of water and one of milk—it contained only four parts of solid matters per 100 parts. One of the contractors who supplied this wretched stuff was a churchwarden of the parish!

Adulteration with Sugar.—Only three cases of adulteration with sugar, and one of adulteration with treacle, have come under my notice. In two of the former cases there were prosecutions by the Corporation of Dublin, but both unfortunately failed. One owing to the death of the defendant before the matter came on for trial; in the other there was a dismissal on account of a technical error made by the food inspector concerned in the case. In one of these cases the adulterants consisted of 90 per cent. of water and 0·5 per cent. of

sugar; in the other specimen of 50 per cent. of water and $2\frac{1}{2}$ per cent. of sugar.

Adulteration with Common Salt.—I cannot understand the object of adding salt to milk, but this substance is occasionally used for that purpose in Ireland. Last year the guardians of the North Dublin Union prosecuted a contractor who had supplied to them milk which I had certified was adulterated with 50 per cent. of water, and, at least, 0·5 per cent. of common salt.

Adulteration with Chalk.—It is a popular belief of great antiquity that chalk is a common adulterant of milk. On the other hand, I am under the impression that public analysts doubt that such a sophistication ever existed. I have to record one, and the only case, that came under my notice. A specimen of milk sent to me for examination by the guardians of Millstreet Union, county of Cork, was found to contain 1·11 per cent. of ash, of which one-half was insoluble in water, dissolved with effervescence in hydrochloric acid, and yielded lime equal to $\frac{1}{2}$ per cent. of carbonate of calcium in the milk. The article was well watered, but the chalk had probably been added to rather stale milk to correct acidity, for the weather at the time was very hot.

Adulteration with Flour.—In Ireland this form of adulteration is excessively rare, as I have met with but one instance of it. Last year I examined a specimen of milk for Mr. Lewis, the well-known perfume manufacturer of Dublin, and found that it contained a very large amount of rice flour.

These are the only adulterants which I have found in milk. When the enormous number of samples which I have examined is considered, the cases of sophistication with articles other than water are extremely few and unimportant, numbering altogether only six, exclusive of salt, which is frequently added to milk. As these cases are authentic, and as similar forms of adulteration, except, perhaps, salt and sugar, have not been noticed by the modern "public analysts," it is worth while placing them on record.

Milk deficient in Fats.—The practice of selling milk which has been deprived by skimming of a portion of its cream is much more common in England than in Ireland; but a very common usage in the latter country is to sell as whole milk that portion of the milk which is first drawn from the cow, and is termed "foreings," or fore milk. The fore milk is very poor in fat, which is the most valuable constituent of the article, whilst the portion of the milk last drawn ("strippings") is extremely rich in fat, and, in fact, is

sometimes like cream. The average amount of fats in good milk is about $3\frac{1}{2}$ per cent., whilst in fore milk it sometimes sinks so low as 0.5 per cent. So common is the practice of selling fore milk for whole milk that when I find a specimen to have less than $2\frac{1}{2}$ per cent. of fats, I certify that it is milk which has been deprived of a portion of its cream by skimming, or that it is fore milk, which practically is the same thing. On this form of certificate there have been many convictions obtained. In August, 1877, a dairy proprietor was fined £10 by a Dublin police magistrate for having sold fore milk as whole milk. He confessed to the practice in court, and added that he sold the strippings as cream.

PRESERVATION OF FOOD, &c.

In hot weather borax is often added to milk, merely for the purpose of retarding its acidity. This salt is also occasionally employed as a preservative agent, especially for meat. G. le Bon, in *Comptes Rendus des Séances de l'Académie des Sciences* for December 2, 1878, states that borax acts as a poison when taken in small and repeated doses. Food preserved by means of borax produces, according to this writer, serious intestinal derangements. Peligot, in his experiments to determine the action of saline substances upon plants, found, sometime ago, that borax exercised an injurious influence upon them.

To detect borax in milk, 5 or 6 oz. of the suspected liquid are mixed with a few grains of carbonate of sodium, evaporated to dryness, and the residue burnt. To the ash thus obtained half a drachm or so of sulphuric acid is added, and the mixture strongly heated, in order to drive off the excess of acid—i. e., that which remains after the decomposition of borax (sodium biborate), and the formation of sodium sulphate and free boric acid. The latter may be dissolved in alcohol, and the solution set on fire in a shallow capsule; the green colour of the flame (and the boron bands, if the spectroscope be used) reveals the presence of boric acid.

A. dal Piaz recommends that fruits should be preserved in a solution of sugar and salicylic acid, in the proportion of from 100 to 300 grammes of sugar, $2\frac{1}{2}$ to 3 grammes of salicylic acid, and 1 litre of water. He states that raspberries, cherries, grapes, and other fruits preserved in this way retained their aroma for a year. (*Biedermann's Centralblatt*, Heft 5.)

It may not be here out of place to direct attention to Mercier's suggestion (*Les Mondes*, November 4, 1878, quoted from *Archives*

de Genève) to preserve animal and vegetable substances and specimens in a strong solution of salt. The brine is boiled to expel air, the specimens are immersed at 80° C., and closed up. Brine is cheaper than alcohol, and is not likely to be drunk by persons in museums where zoological specimens are preserved.

IS TUBERCULOSIS COMMUNICABLE?

The notion that phthisis is contagious is one which has long been entertained by both medical and non-medical persons. It has often been noticed that persons after a long-continued attendance upon phthisical patients become phthisical themselves. Very frequently the attendant on a sufferer from phthisis is a sister or other near relative, and when the latter becomes affected with the disease it is accounted for by the fact that, as the brother or sister whom she had attended had the same disease, it must be "in the family." No doubt phthisis is a disease which often attacks the members of particular families during several generations, even when they are not living together; but if it be true that the disease is contagious, it must sometimes happen that when more than one member of a family contracts it, contagion, and not, as is almost always assumed, heredity, is the cause of the second case.

In Virchow's *Archiv* for November, 1878, this subject is considered in a paper by Dr. Tappeiner, of Meran. He placed dogs (these animals rarely suffer from tuberculosis) in a chamber in which fine particles of sputum obtained from phthisical patients were suspended. The animals, eleven in number, after breathing for several hours daily this infected air, were killed and examined. In ten cases the lungs were found to contain well-developed miliary tubercles; in most of the cases there were tubercles in the kidneys, and in a few instances in the liver and spleen.

The sputum used was very small in quantity—in three cases merely one gramme daily. It was dissolved in water, and diffused through the chamber by means of a "steam atomiser."

Dr. Max Schottelius repeated Tappeiner's experiments, but, in addition, employed the sputa of persons affected only with bronchitis, and he also used pulverised brains, cheese, and cinnabar. The results were that both the phthisical and bronchial sputa produced miliary tubercles in all cases, and that these were even caused, but to a far less extent, by the powdered brains and cheese, especially the former. The cinnabar dust produced a few whitish tubercles with coloured centres, with an interstitial deposit of the cinnabar,

without apparently any inflammatory result. It is remarkable that all the animals experimented upon remained in good condition, and apparently in good health, during the entire progress of the experiment.

Tappeiner, in two instances, caused dogs to inhale air containing "anatomised" cheese, but no change in the lungs resulted therefrom.

Although the results of Tappeiner and Schottelius agree in some respects, they do not accord in other important particulars, as above shown. It is further noteworthy that two dogs at Munich, to which phthisical sputum had been given in their food, were found to have tubercles in their lungs, whilst the lungs of six dogs at Meran, also fed with substances containing sputum, were found to be normal.

These experiments require to be repeated upon a larger scale, and by many observers, before we can accept the conclusion that the inhalation of phthisical sputum produces tuberculosis. One thing appears, however, to be clearly proved—not merely by the results of Tappeiner's and Max Schottelius' experiments, but also by those of hundreds of observations made by others—namely, that the inhalation of solid matters produces tubercles. It is remarkable that the hard mineral powder used by Schottelius did far less damage than the soft cheese and brains. Are we to infer from this that organic matter, probably in a decomposing state, acts more injuriously on the lungs than a mineral substance does? Whilst we can readily understand that the sputum of a phthisical patient might induce tuberculosis if introduced into the lungs of a healthy person, the difficulty is to understand why a few "atoms" of cheese should do so. In short, the whole subject now under discussion may be regarded as unsettled.

METALS IN RELATION TO THE COOKING AND PRESERVING OF FOOD.

In *The Lancet* for July 3rd, 1878, Mr. Hartley states that a certain amount of danger attends the use of "tinned fruits." Peaches, apricots, and other fruits are largely imported from America. They are placed, immediately after being plucked, in tins, which are then filled with syrup and hermetically closed. The acid of the fruit attacks the metal and dissolves it. In the syrup from a tin of one pint capacity, in which pine-apples had been preserved, Mr. Hartley found one grain weight of tin. In a large tin of

apples four hundredths of a grain of the metal were found. Mr. Hartley recommends that the syrup should be thrown away, though the fruit may be used.

We have little, if any, reliable information in reference to the action of small quantities of tin upon the animal economy, but the metal is certainly not an active poison. According to Pliny the Ancients were not afraid to use this metal: "*Stannum illitum aeneis vasis saporem gratiorem facit, et compescit æruginis virus.*"

The *Annales d'Hygiène Publique et de Médecine Légale* for November, 1878, contains an article by M. Galippe on the use of copper vessels for cooking in. He states that the objection to the use of this metal in the kitchen has not arisen from a popular prejudice, but is the result of the teachings of the scientists, some of whom have preached a crusade against the metal. He refers to the fact that Queen Christina of Denmark caused a statue to be erected to Professor Schoffor in recognition of his efforts to prevent the use of copper for culinary purposes, and her prohibition of the employment of the metal in the manufacture of cooking utensils, although copper was one of the sources of the national wealth.

M. Galippe utterly discredits the current notion that food cooked or preserved in copper vessels is unwholesome; and so far as he himself is concerned, his proposition may be considered as proved. For the greater part of fourteen months he subsisted upon meat, fish, and vegetables, prepared in copper vessels according to the methods commonly in use. Acid fruits were cooked and conserved in copper vessels, allowed to cool therein, and to remain for variable periods of time. M. Galippe states that neither he nor those who were the inmates of his house, or accepted his hospitalities (men, women, and children of all ages), suffered in the slightest degree from the use of the food prepared in the copper vessels.

M. Galippe admits that two slight drawbacks (*inconvenients*) occasionally attend the use of copper cooking vessels. One is, that fatty foods allowed to cool in such vessels often acquire a green colour very readily. It is this green colour (*vert de gris*) which frightens so many people when they see it in copper vessels; but according to M. Galippe it is merely an optical fact, and he asserts that fatty substances coloured with *vert de gris* are innocuous.

Copper culinary utensils are sometimes coated with tin, with the view of preventing the food cooked therein from absorbing the copper. M. Galippe denounces this practice, and contends that the

tin is likely to be dissolved by the food. Tin, no doubt, is not an active poison, when pure; but as it is liable to contain lead—from 5 to 55 per cent.—there is real danger to be apprehended when certain kinds of food, solid and liquid, are cooked in tinned utensils. Water containing common salt, and other salines, when boiled in vessels composed of alloys of tin and lead, dissolve a sensible proportion of these metals, especially the latter.

Copper has been detected by many chemists in large numbers of animals and plants, and it appears to be frequently, if not constantly, present in the former. The eminent French sanitarian, Chevallier, seems to have had a great prejudice against copper cooking utensils, for he denounced^a the use of cooking vessels made of that metal. He believed that very small quantities of copper produced toxic effects. On the other hand, Bourneville gave 1,000 grains of sulphate of copper to an epileptic patient within 151 days. The patient died from other causes, and the autopsy showed that the copper had produced no effect upon the mucous membrane of the intestinal tract. Levi and Barduzzi gave large doses to men and the lower animals, and found that it was not only tolerated but that it produced a remarkably good effect upon the processes of digestion. Lastly, some excellent mineral waters—for example, those of Wiesbaden, Aix-la-Chapelle, Bagnères, &c.—contain sensible amounts of copper.

WATER PURIFICATION.

Several experiments have recently been made with the view of determining the relative and absolute efficiency of some of the water-filters now in general use. Mr. G. W. Wigner, F.C.S., a well-known London analyst, has published (*The Analyst*, Oct., 1878) the results of his experiments with “spongy iron” and “silicated carbon” filters, of which immense numbers appear to be employed in England. He filtered water of known composition through each filter and through both. The results obtained will be found in the accompanying table.

^a A. Chevallier. *Annales d'Hygiène*. 1843 and 1850.

TABLE.

	No. 1. Tap in Main.	No. 2. Silicated Carbon Filter.	No. 3. Spongy Iron Filter.	No. 4. 1st. Silicated. 2nd. Spongy.	No. 5. 1st. Spongy. 2nd. Silicated.
Colour - -	Yellow-green, slightly opaque.	Pale blue.	Pale blue.	Opaque blue.	Chalky blue.
Suspended matter -	Traces.	None.	None.	Traces.	Slight traces.
Smell - - -	Offensive.	Very slight.	Offensive.	Decidedly offensive.	Decidedly offensive.
Taste - - -	Slightly of- fensive.	Saline.	Saline and offensively flat.	Unpleasant and very flat.	Excessively flat and ob- jectionable.
Hardness before boiling	27·2°	16·9°	11·6°	14·0°	17·1°
Hardness after boiling	9·4°	5·3°	9·3°	10·7°	7·2°
Total solids - -	69·60	66·90	58·35	60·70	69·75
Loss on ignition -	5·80	6·80	5·85	4·80	9·40
Total mineral matter -	63·80	60·10	53·00	55·90	60·35
Chlorine, as chloride of Sodium.	36·66	36·66	36·46	36·50	36·50
Nitrogen, as Ammonia	None.	None.	·0728	·0875	·0149
Do. as albuminoid Ammonia	·0083	None.	·0161	·0140	·0051
Do. as Nitrates -	·7500	·7300	·6600	·6600	·7300
Do. as Nitrites -	Traces.	Traces.	Traces.	Traces.	Traces.
Total Nitrogen in these four forms.	·7523	·7300	·7489	·7615	·7500
Oxygen absorbed from Permanganate.	None.	None.	·0630	·0314	·0153
Microscopical examina- tion of residue.	Very bad animal and vegetable debris. Some small spores. Quartz.	Organic debris, minute traces only. Mineral matter, traces.	Satisfactory. It was very free from suspended matter.	Satisfactory.	Satisfactory.

These results show that the silicated carbon filter purified the water better than the spongy iron did; and the curious result is obtained that no advantage followed the filtration of the water through both filters. What strikes us as remarkable in these experiments is that filtration first through the silicated carbon, and secondly through the spongy iron, actually increased the total amount of nitrogen in the original water. This could only be accomplished by the conversion of the nitrogen of the air dissolved in the water into ammonia. The silicated filter removed the flavour and much of the odour of the water, whilst the "spongy iron" altered, but not for the better, the nature of the odour of the water, and also rendered it flat and somewhat disagreeable to the taste.

As other experimenters have reported favourably in reference to the efficacy of the "spongy iron" filters, we should like to have a repetition of Mr. Wigner's experiments before condemning a form of water-purifier now in very general use throughout England.

THE EUCALYPTUS.

A tree of lofty stature was discovered in the year 1792 by a French naturalist, Labillardière, in Australia, during the celebrated voyage in quest of *La Perouse*. It is stated that the tree, which belongs to the myrtle family, rivals in size the celebrated mammoth trees of California, attaining to a height of from 400 to 500 feet. The bark (which is very thin), the leaves, and the fruit, are highly aromatic. The bark is used as a remedy in intermittent fevers, and in other maladies. The exhalations from the tree are stated to be highly anti-miasmatic. It has been naturalised in Algiers, and in seven years a forest of trees, 50 feet in height, were produced. The atmosphere in this forest is described as having a most agreeable balmy odour.

Doctors Mosler and Goeze have published an account of the results which have followed the planting of the eucalyptus in the city of Greifswald. Malarious diseases were common in this city until the moats which surrounded it were filled up, and its old walls—once used for defensive purposes—demolished. The city is still, however, subject, but to a far less extent, to malaria. Mosler and Goeze planted in pots cuttings of the eucalyptus last winter, and in the following summer removed them into the garden of University Hospital, where they thrived very well. Twelve cuttings of from 2 to 3½ inches in height were transplanted in June last, and at the beginning of November they had attained to a height varying from 3 to 5 feet,

and presented a healthy appearance. These plants have been taken into winter quarters, which consist of the wards of the hospital. It is proposed next summer to put them out into the open air again, and it is expected that after three or four years they will become sufficiently hardy to remain in the open air during the winter, notwithstanding the coldness of the climate and the prevalence of the biting north winds from the Baltic. It is stated that the plants kept in the wards evolved an aromatic odour which proved very grateful to the patients, as it neutralised the unpleasant smells which were simultaneously developed from various sources. It appears that many inhabitants of the city have secured specimens of the plant and keep them in their apartments as air-purifiers.

The eucalyptus grows so rapidly that a plantation might be formed in three or four years, provided that climate and soil were favourable. Dr. Brady, Inspector of Anatomy, has a fine healthy plant growing close to Dublin; and no doubt the tree would flourish in Ireland, owing to the mildness of the winter. In England only a few attempts to acclimatise the eucalyptus have succeeded. It would be well to make an experiment in the new park, which is taking the place of St. Stephen's-green, Dublin; for if the eucalyptus were acclimatised in the parks, squares, and gardens of Dublin, the result *might* be an improvement in the sanitary state of the city.

DEATHS FROM EATING MOULDY BREAD.

A remarkable case of poisoning was recorded by Mr. A. H. Allen, Public Analyst for Sheffield, in *The Analyst* for November, 1878, and was also referred to in many of the journals, lay and professional, about the end of October and beginning of November. A Mr. Thresh kept a cheap eating-house in Barnsley, and on a particular day his cook prepared two bread puddings, consisting of scraps of stale bread, sandwiches, milk, eggs, sugar, currants, and nutmegs. The bread—accumulations of some weeks—was steeped for a night in cold water before being incorporated in the puddings. These were baked in separate ovens. We may distinguish the puddings by the numbers 1 and 2. No. 1 was partaken of by the cook, the proprietor of the eating-house, a waiter, and a customer. The remainder was used by four children of Thresh's, who lived in another house. All of these eight persons were seized with violent vomiting and purging. One, Ellen Thresh, aged three years, died in 36 hours, and another, Mason, a waiter, succumbed in a week. The child was delicate and subject to diarrhoea, and the man was

weakly and of intemperate habits. No. 2 pudding was only partaken of—and in very small quantity—by one person, who did not suffer in any way from its use. The portion of the pudding which was not eaten, the rest of the currants used in making the pudding, and the spoon used in helping the food, were examined by Mr. Allen, who failed to detect any known poison in or on them.

Mr. Allen consulted several toxicologists, myself included, as to the probable cause of the poisonous properties of the pudding—for no doubt could be entertained as to the fact of the noxious character of the pudding. Dr. Tidy suggested that there must have been ergoted grain used in preparing the flour; but the symptoms which the poisoned persons exhibited were rather inconsistent with the theory of ergot poisoning. I suggested that a poisonous fungoid growth had taken place in the scraps of bread, some of which had been kept for some weeks.

A microscopic examination failed to detect evidence of the presence of ergot. Mr. Allen came to the conclusion that the poisonous effect was due to the presence of a microscopic fungus. He succeeded in producing in bread soaked in milk and water, by mixing with it a little of pudding No. 1, a body which he considered to be fungoid, and which gave an ergotoid reaction with soda. It is known that on treating ergot with solution of soda, a red colour and an odour of herrings (that of trimethylamin) are developed. Allen found that these reactions were equally produced by the action of soda on moulds; and he further found that the bread in which he had cultivated moulds caused a mouse to which it was administered to be violently purged.

There are on record some cases of alleged poisoning by eating mouldy bread; in Von Boeck's new work on poisons, one is mentioned in which three persons were very ill, and one, a child, died.

In this journal I published a case of the poisoning of a whole family, which I was clearly able to refer to the use of highly rancid and mouldy butter; one of the family, a little girl, died.

One day in 1877 the wife and children of a gentleman, well known in Dublin, partook of a luncheon, consisting of bread, butter, and lobster salad. Immediately afterwards all of them were seized with severe vomiting and purging. Dr. Chapman, of Pembroke-road, promptly treated the cases, and with success, though one of the patients became first delirious, and then sank into almost complete coma. Dr. Chapman kindly invited me to see the cases, and asked me to examine the remains of the lobster and the tin in which

the fish (for it was preserved lobster) had been contained. Poisonous metals were proved absent; the lobster was not putrescent in the slightest degree, but it had a peculiar and unmistakably mouldy odour. Examined under the microscope it was found to contain large numbers of bacteria and microscopic fungi, and to the latter there is no doubt the poisonous properties of the preserved lobster were due.

In the *Veterinarian* for May, 1878, I recorded the poisoning of a large number of oxen and sheep, which I had traced to the use of mouldy food (an artificial feeding cake).

Dr. Muter records, in *The Analyst* for November, 1878, a case in which the family of a gentleman suffered from sickness and purging after partaking of "the usual morning's milk." He found that the milk had a foul odour, and what appeared to be numerous fungoid cells. The can which contained the milk was examined, and fungoid growths and bacteria were found adhering to the joints, which were filled with decomposing milk constituents. Dr. Muter concluded from these observations that the filthy vessel contained a septic material—probably of a poisonous fungoid nature—which was propagated in the milk. I fear much that milk vendors are not very particular in reference to the cleansing of their pails and cans; they certainly are not remarkable for clean hands and well-brushed clothes.

Mr. W. Thompson, of Manchester, commenting (*Analyst* for January, 1879) on Allen's and Muter's papers, refers to some investigations made by the late Dr. Grace-Calvert and himself. When eggs were left under certain conditions, some of them putrefied, but when examined microscopically were found to contain, not fungoid growths or bacteria, but numerous cells of different sizes and similar shape. These cells developed others, in the same way as the yeast plant, and when present in large numbers emitted a putrid odour. They appeared to be morbid developments of the bioplast of the yolk which should have gone to form the bone, flesh, &c., of the chickens. An experiment made on a healthy dog appeared to show that these cells were poisonous. Mr. Thompson suggests that these "putrid cells" may have been the cause of the poisonous properties of the eggs in Mr. Allen's case, and of the milk in Dr. Muter's.

PRIVY DISINFECTION.

According to A. Eckstein, chloride of lime is a better disinfectant (deodoriser?) for privies than any of the agents used for that

purpose. He found that the aqueous solution of one kilogramme of sulphate of iron destroyed the odour of a privy used by 100 persons, but the effect lasted only twelve hours. Sulphate of copper acted similarly. One kilogramme of solid sulphate of iron lasted for two days. One kilogramme of a mixture of sulphates of iron and copper with carbonate of lime kept down the odour of sulphuretted hydrogen for two days. Liquid sulphurous acid acted rapidly, but its effects lasted for less than twenty-four hours. Crude carbolic acid masked the odours of the privy, but with another odour only somewhat less disagreeable. The use of parchment paper prolonged the action of the deodorant. Thus, one kilogramme of sulphate of iron (*copperus*), contained in a parchment paper bag, began to operate after two hours, and acted during three days; one kilogramme of good chloride of lime, contained in a parchment wrapper, kept the privy inodorous for a period of nine days.

MUCOID ALGÆ IN POTABLE WATERS—TANNIN A TEST FOR WATERS.

Dr. H. Kämmerer has suggested the use of tannic acid as a reagent for water analysis. He points out that the occurrence of glue in subsoil waters is undeniable, and that as tannic acid precipitates glue and kindred substances, it should always be used in the sanitary examination of water. He is of opinion that any water rendered decidedly turbid by the addition of tannin to it is unfit for use. As the presence of saline matters and other compounds retard the precipitation of glue, &c., by tannin, the water must be allowed to stand for twenty-four hours before a decision as to the effect of tannin upon it is arrived at.

Dr. H. Hager, referring (*Biedermann's Centralblatt*, July, 1878) to Kämmerer's statements on this subject, mentions the following interesting fact:—In 1866, when cholera was prevalent in Berlin, he resided in a district ravaged by the disease. On examining the waters of the district, he found that the bad ones could be rendered drinkable by the addition of a solution consisting of 2 parts of tannin, 1 part of sugar, 5 parts of alcohol, and 3 parts of water. The precipitate produced by the tannin was at first supposed to be glue, but microscopic examination proved that it consisted of algæ chiefly belonging to the *Oscillaria*. It is probable that Kämmerer's "glue" and Hager's mucoid algæ may be identical; but, in either case, the tannin is a good test for water analysis.

PART IV.
MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL
SOCIETY.

FORTY-FIRST ANNUAL SESSION.

EDWARD B. SINCLAIR, M.D., President.
WILLIAM ROE, M.D., Honorary Secretary.

Saturday, January 4th, 1879.

E. B. SINCLAIR, M.D., President, in the Chair.

Spontaneous Inversion of the Uterus induced by the presence of a Fibroid springing from the Fundus. By LOMBE ATTHILL, M.D.; Master of the Rotunda Lying-in Hospital.

ON the 27th of August last a patient was admitted into the Rotunda Hospital, on the recommendation of my friend, Dr. O'Sullivan of Cork, who has kindly furnished me with the following note of her case:—

“M. S., aged twenty-one years, unmarried, was admitted to Cork North Infirmary, under my care, on the 18th January, 1878. She was sent to me by Dr. Cremen for operation, his diagnosis being that she was the subject of a fibroid polypus of the uterus, projecting into the vagina. For many months previously she had suffered from constant hæmorrhage from the uterus, and had become quite blanched and anæmic. She also stated that some months ago she had experienced very severe pains, resembling labour pains; subsequently the tumour was found in the vagina. Upon examination per vaginam, I found the tumour as described, with what appeared to be a long, but rather thick pedicle, apparently attaching it to the os uteri, which latter could scarcely be felt. Unfortunately for the correctness of the diagnosis, I did not attempt to pass a sound into the uterus, or make a recto-vesical examination, so evident was the outline of the tumour. The tumour appeared to be surrounded near its base by a slight sulcus. After consultation with Dr. Hobart, one of the surgeons

of the Infirmary, we decided on removing the tumour with the wire ecraseur. On the 22nd January, assisted by Doctors Shinkwin, Hobart, and Cremen, I proceeded to operate. Having drawn the tumour well down into the vagina, I applied the wire of the ecraseur around the "pedicle," and commenced to work the instrument slowly. I used no anæsthetic, and was very glad I had not done so, as the patient immediately complained of excessive pain when the wire began to tighten, and the pain became so great that we considered it prudent to stop the operation and to remove the ecraseur; and, upon making a further and more careful examination—having first drawn the entire tumour outside the vagina, which was easily done—I discovered that the tumour was an inverted uterus, with a fibroid growing from the fundus, such as those described by M'Clintock, Sayre, and others. There was no pedicle, but a slight groove could be observed at its attachment to the uterine wall. The wire of the ecraseur had commenced to cut through the cervix uteri posteriorly, and from the wound there was a slight hæmorrhage, to which I applied the liquor ferri perchloridi. I returned the tumour with the uterus into the vagina, and, fearing hæmorrhage, plugged; and, with a view to avert peritonitis, opium was freely administered. For several days she continued very ill, vomiting much, but under treatment all inflammatory symptoms subsided, and she gradually got better, recovered her appetite, and with it renewed health; colour was restored to her lips and cheeks, and she put up flesh; there was no further hæmorrhage, but there continued for a time a foetid discharge from the vagina, which was counteracted by injections of Condly's fluid. Menstruation did not return for six weeks, and lasted only a few days. She was discharged from the Infirmary on the 12th April, 1878, and returned to the country, where her health appeared to be gradually improving, until the hæmorrhage again returned, and continued from time to time with more or less severity up to the date of her admission to the Rotunda Hospital."

On admission this patient presented a very anæmic appearance, the pulse was feeble, and she complained of being very weak. She stated that she was quite free from pain, but suffered from an almost constant sanguineous discharge. On making a vaginal examination, a tumour as large as a man's fist was found lying in the vagina; it could be traced up to the os uteri, which encircled it, but the sound did not penetrate to the depth of more than half an inch within the os, and a bimanual examination showed that the body of the uterus was absent from its normal position in the pelvis; it was therefore evident that Dr. O'Sullivan's diagnosis was perfectly correct.

The first step obviously was to remove the tumour; this was effected on the 29th. The patient being etherised, and the whole tumour having been drawn outside the vulva, it was detached, or, more correctly, enucleated, the finger and handle of a scalpel being alone used for the purpose. A

good deal of bleeding followed, but the raw edges were drawn together with catgut sutures, and the inverted uterus being pushed back into the vagina, the hæmorrhage quickly ceased. No attempt was at this time made to re-invert the uterus. I deemed this inexpedient for several reasons. The presence of the extensive lacerated wound, caused by the removal of the tumour, was alone sufficient to deter me from active interference; besides, Schroeder affirms that after the detachment of the tumour, the uterus will re-invert itself. It was, therefore, right to give time to see if this would occur, besides we had reason to fear that adhesions might have formed during the previous attack of peritonitis; the breaking up of these in her then weak state would have been a very serious matter.

After the removal of the tumour, the patient rapidly improved; there was no return of the hæmorrhage, her appetite became good, and she put up flesh; but an examination made at the end of a fortnight proved that the uterus remained as large and as completely inverted as ever. She was now allowed to be up, and from this date she rapidly gained strength; there was not any discharge, nor did menstruation occur.

Five weeks thus elapsed, and then, no change having taken place in the condition of the parts, I decided to attempt to re-invert the uterus. Accordingly, on the 4th October, the patient having been brought under the influence of ether, I introduced my right hand into the vagina, and steadily compressing the fundus with my fingers, endeavoured at the same time, with the palm of my hand, to force the inverted part through the os, but without producing any effect; a similar effort made with my left hand had no better success, and finally I abandoned the attempt for that day. She rapidly recovered from the effects of the ether, and did not subsequently suffer any pain. Menstruation appeared about a week subsequently, and was moderate in quantity.

But though I had failed as yet to re-invert the fundus, I was far from despairing of success; indeed the impression left upon my mind, as the result of the attempt I had made was, that I would have succeeded if only I could have fixed the uterus and prevented it receding as I made pressure; this with my hand in the vagina I could not do, and I resolved to make another effort, conducted in an entirely different manner. First of all I had this instrument made for me, which you see is not unlike a stethoscope, only stronger, the extremity being cup-shaped to fit the inverted uterus. The patient was then etherised, and the perinæum being drawn well back by means of a duck-bill speculum, I seized both the anterior and posterior lips of the os uteri with vulsellums; that holding the anterior lip I held myself; an assistant, the one grasping the posterior lip; the larger end of the repositor was then applied to the fundus uteri, the small to my chest; my left hand was, therefore, free, and while I exerted force with the repositor by the pressure of my chest against it, I was enabled to keep the fingers of my left hand in contact

with the os, and thus to judge accurately the impression made on the fundus, and at the same time regulate the amount of force employed; the force used was exercised from my chest, against which the stem of the instrument mentioned rested.

At first no effect was produced, but after a time I perceived that a portion of the inverted mass slipped up gradually; the movement became clearly perceptible; then the fundus followed, and quickly passed inside the os. I, however, still kept up the pressure, fearing the re-inversion would not be complete. After a little time I slowly withdrew the repositor, and passing my hand into the vagina, and my finger into the uterus, satisfied myself that the restoration of the organ to its normal shape had been perfectly effected.

For some days subsequently I felt much anxiety, fearing that peritonitis might occur, but my fears were groundless; no unpleasant symptom followed, and after the lapse of a week I allowed her to get up. The condition of the uterus a few days subsequent to the operation was very similar to that presented by patients in whom the uterus remains imperfectly involuted after delivery. The uterus was very large and soft, and the sound showed that it measured nearly $3\frac{1}{2}$ inches in depth; the os was so patulous that it easily admitted the finger, and there was copious uterine catarrh—in fact, had this patient been then seen for the first time, the recent occurrence of pregnancy would undoubtedly have been suspected.

With the view of stimulating the uterine muscular tissue to contract, I applied carbolic acid on several occasions to the whole of the intra-uterine surface.

On the 23rd November menstruation appeared—this time rather profusely, the loss being much more considerable than on the previous occasion when the fundus was inverted. This certainly was a remarkable fact, and not in accordance with what would have been anticipated.

Although the patient's general health had markedly improved, the uterus reduced very slowly in size, and the uterine catarrh continued copious; tincture of iodine was substituted for the carbolic acid, and subsequently fuming nitric acid was applied through my cannula to the whole of the intra-uterine surface. Neither application caused pain, but the result was not marked; finally, 10 grains of sulphate of zinc were introduced through Simpson's *porte-caustique*, and the patient was put on arsenic, and at the end of December the uterus measured but $2\frac{1}{2}$ inches in depth; the os, however, was still patulous, and its lips abraded. On the 31st December menstruation recurred, and lasted several days—not, however, profusely.

The method I employed in this case differs materially from that recommended by Barnes and other authors, and is so simple, and in the

present instance was so satisfactory, that I think it well worthy of trial in similar cases.

DR. ROE.—What time elapsed between the removal of the tumour and the replacement of the uterus?

DR. ATTHILL.—The tumour was removed on the 29th of August, and the uterus was not re-inverted until the middle of October—about two months. I allowed as much time as possible, in the hope that it might re-invert itself.

The PRESIDENT.—Had she ever been pregnant?

DR. ATTHILL.—No. The latter part of the case is given in Dr. O'Sullivan's letter. She came under his care on the 18th of January last, and the uterus was inverted then, and must have been inverted long before that. It was then diagnosed as a polypus. Not many months previously she had been suffering from constant hæmorrhage of the uterus. It may be inferred that the inversion of the uterus took place about August, 1877, and that it had been inverted about twelve months. I am not entitled to any credit for the diagnosis which was made beforehand. Dr. O'Sullivan very candidly admitted that he had made an erroneous diagnosis. The case was sent to him as one of polypus; but the pain caused by the operation enabled him to detect his error.

DR. MACAN said that through the kindness of Dr. Ryan he was fortunate enough to be called to see a case which very much resembled this. There was a slight difficulty in the diagnosis of the case to which he referred, for Dr. Ryan, after making a most careful examination, arrived at the conclusion that there was a fibrous tumour; but, on the other hand, he could not find the fundus of the uterus, nor could he make out where the uterus began, and the supposed fibrous tumour ended. Then he thought it was an inversion, but there were some things which that hypothesis did not explain either. When he (Dr. Macan) saw the case the tumour was hanging out two inches beyond the vagina, the lower part of it being in a state of slough, so that antiseptics had to be used in order to keep down fœtor. The tumour seemed like a polypus, and could be pinched without pain, but the moment the uterus was pinched the woman cried out. In her case the history of hæmorrhagic affections went back fourteen years, and she was sometimes nearly at death's door in consequence of them. There was a stoppage of the hæmorrhage six years previously, and it seemed to him that that must have been the time when the tumour came through the orifice of the uterus, the hæmorrhage not being so great as it had been previously. The tumour was removed without difficulty. At first he thought of incising the capsule, but found that it bled freely, being full of vessels. Then he determined to put on an ecraseur, and did so, removing the tumour. Subsequent to the operation he found that hæmorrhage,

though not by any means profuse, went on, and that he could not stop it with perchloride of iron, although the bleeding surface was not much larger than a shilling. Dr. M'Clintock had a case in which the hæmorrhage could only be stopped by constant pressure of the finger. In his own case—as he wanted, in the first instance, to reduce the inversion of the uterus—he put on three ligatures, and drew the surfaces together, so that not a drop of blood could escape. He then tried to reduce the inversion, but utterly failed. The woman was forty-six years of age. Dr. Ryan also tried to reduce the inversion, but in vain. Then Dr. Ryan said that if he (Dr. Macan) did not perform the amputation he would do it himself, as he considered the woman's life in danger. He (Dr. Macan) accordingly took off half of the uterus, and then brought the edges together by sutures as completely as if the peritoneal surfaces were in apposition. There could then be no hæmorrhage, while there was a chance of adhesive peritonitis between the two surfaces. Afterwards the woman menstruated, and no one could tell that anything had happened to her at all. He met with another case of inversion in the Rotunda Hospital, in which the uterus was taken off, and yet the woman had not a single bad symptom afterwards. It had been said that an inverted uterus generally went back of itself. Why it should go back after a tumour, when it would not go back after delivery, he could not understand. In Dr. Atthill's case there was no effort at re-inversion. With regard to the methods of re-inversion, he believed the best was to make a pressure at the part of the uterus where the Fallopian tube went in. Dr. Emmett, of New York, said that when the fundus was got to the level of the external os, the difficulty generally was to get the former up the cervix. In such cases what he did was to put two silver sutures through the lips of the external os to prevent the fundus from going farther down, and then a spontaneous re-inversion took place by the constant pressure of the silver sutures on the fundus. These cases were all mentioned in a paper that he (Dr. Macan) had read before the Society. There was one in which inversion occurred in a woman who had been a widow for six or seven years, and who had suffered from menorrhagia. And some German authorities were of opinion that inversion depended greatly on where the site of the placenta was.

DR. M'CLINTOCK once met a case of complete inversion of the uterus, caused by a small polypus, not larger than a Spanish chestnut, which was adherent to the fundus. The woman was between sixty and seventy years of age. He relieved her of her trouble by two operations, though he afterwards found that one would have sufficed. First he removed the tumour from the extremity of the inverted fundus with an ecraseur, and a most troublesome hæmorrhage ensued, which was very difficult to control, as was generally the case in hæmorrhages from the substance of the uterus, because the arteries had not much space to retract. Though

the extent of surface exposed was very small indeed, the hæmorrhage was most troublesome. He made no subsequent attempt at reposition, thinking that it would be utterly useless. He secured it with ligature, which he left on for twenty-four hours; and the woman afterwards recovered perfectly. The idea of some mechanical support, such as Dr. Atthill had suggested, had occurred to other minds also. In a case he (Dr. M'Clintock) had in the Lying-in Hospital, it occurred to him to try to push up the uterus. He got a brass cup made, with a metal handle, the cup being made of sufficient size to receive the uterus. He tried the instrument, but it failed, as did every attempt at reposition, though the case seemed a most inviting one for it, as the uterus was very small. That was a case of inversion following delivery, and eventually he had to remove the uterus. A few days ago he received from New York a paper by Dr. John Byrne, of that city (who held a high position in the Brooklyn Hospital), which was published in the last volume of the *American Gynæological Transactions*. The author described a novel mode of reducing chronic inversion of the uterus. In principle it was essentially the same as that employed by Dr. Atthill, but the instrument, which Dr. Byrne called a repositor, was a more artistic, and probably a more efficient one for the purpose than that extemporised by Dr. Atthill, though no better results were obtained from it. The most important part of the treatment recommended by Dr. Byrne was the use of strong contrary pressure on the hypogastrium over the fundus of the uterus, in order to save the vagina from enormous stretching and consequent laceration. Dr. Atthill overcame that danger by catching the os uteri and holding the uterus down. Dr. Byrne and Dr. Gaillard Thomas had a cup-shaped instrument for making pressure on the hypogastrium; and with it Dr. Byrne succeeded in reducing an inversion fourteen days after delivery, although that was not a great triumph. In a case which he (Dr. M'Clintock) attended with Dr. Churchill, some years ago, he replaced the uterus with the hand eleven days after delivery. Dr. Byrne's repositor was also used by Dr. Thomas, in a case of long standing, with most satisfactory results. In Dr. Atthill's case the symptom of pain which occurred on the compression of the uterus by the ligature proved that that symptom, which was relied on by the older writers, was a valuable diagnostic mark.

DR. DENHAM said he remembered very well the case of the entire removal, by ligature, of the uterus, alluded to by Dr. M'Clintock; it occurred in the first year of his (Dr. Denham's) Mastership, upon an occasion when Dr. M'Clintock undertook the charge of the institution for him. The only point he desired to refer to was the smallness of the tumour, as not being sufficient to account for the inversion. He looked on that as an accidental circumstance. Dr. M'Clintock had given a beautiful drawing of that case in his work and anyone referring to it

would see that the tumour, which was removed with an ecraseur, was not much larger than a filbert, and that it was at the fundus. They had many of them seen polypi of much larger growth, which did not induce inversion. However, it mattered very little what the cause of an inversion was. He believed it would be found that considerable pressure on the body of the inverted uterus would very much facilitate its return. Dr. Atthill had been very successful in the use of the instrument he had shown; but still there might be many cases in which that instrument might not be so successful in the hands of others. In many cases a pressure upwards would be likely to succeed by the simple brute force of putting the instrument against the inverted body and then pressing with the chest. At the same time they should not be disappointed if in other cases like that described by Dr. Atthill the operation should fail. In a former report made to the Society, he (Dr. Denham) mentioned a case of inversion induced by a midwife, who was in a great hurry to complete the labour before the arrival of the physician. She forcibly pressed on the fundus and at the same time dragged the cord, and by so doing completely inverted the uterus. This was concealed by her for some time, until the woman was sent to hospital, almost dead from hæmorrhage. He applied upward pressure and succeeded in effecting re-inversion to a certain extent; but a small portion of the fundus remained like an India-rubber ball, and, dreading that too much pressure would produce inflammation and injury to the parts, he let the case remain so until the following day, when they found that nature had completed the operation, the fundus having slipped up of itself. The woman was in delicate health—nevertheless she afterwards conceived, and came to the hospital, where she died of puerperal fever, which was then epidemic.

The PRESIDENT stated that a case of uterine inversion, occurring at delivery, and not discovered until eight years afterwards, came under his observation in Sir Patrick Dun's Hospital. Dr. Denham, he thought, saw the case with him. They tried the taxis, which led to great hæmorrhage and such exhaustion of the patient that they desisted, and he was very glad they did. She permitted him to remove the tumour. He used Gooch's double cannula and waxed whip-cord, with which he snared the uterus. The great point in snaring was to completely strangulate the parts at once. He did so, and profuse hæmorrhage from the uterus immediately followed, but lasted only a very short time. No very serious symptom ensued; and, at the end of the fifth day, the lower portion of the uterus below the ligature was completely decomposed. He removed that portion of the uterus with a scissors below the ligature. The stump, with ligature attached, came away in forty-eight hours afterwards. When he examined the tumour, he found that the peritoneal cavity was nearly obliterated by adhesion; the appendages on one side were annihilated, and there was only a trace of the ovary on the other. Had he

continued the taxis, he believed that the case would have terminated fatally. The woman left the hospital perfectly well, and is at this moment enjoying excellent health. He preferred ligatures and waiting until the part below was in a state of decomposition, taking care to use injections in order to prevent septicæmia. Under such management adhesions above the ligatures would, in his opinion, be more effectual and certain.

DR. ATTHILL, in reply, said cases of the sort were very rare, perhaps not occurring more than once in a man's lifetime. With reference to the observations of Dr. Denham, the paper showed that he (Dr. Atthill) tried taxis until both his hands were thoroughly paralysed. In order to re-invert, they required pressure of some kind. To get the uterus fixed upon the abdominal wall was almost an impossibility, especially when the uterus was small. The mass of intestines and the thickness of the abdominal wall rendered it almost impossible to get the hand deep into the pelvis. Again, when the uterus had gone down through the os, there was nothing for them to make contrary pressure. They might make pressure on the abdominal wall and yet little or none on the fundus itself. The whole stress was, consequently, thrown on the vagina, which would resent such pressure; and, in the case detailed, if he had not succeeded in fixing the uterus as he did, he would not have succeeded at all. Dr. Macan had mentioned that some German authorities considered that the great difficulty was to get the fundus beyond the os. He (Dr. Atthill) thought there was reason for saying that that was because there was not proper means of doing it. When he tried taxis with his hand, he reduced the bulk of the uterus very considerably, and thought he would have been able to send it through the os; he did send it a little way, but could not afterwards follow it with his fingers. An instrument such as he showed would be very useful as a means of following the uterus and overcoming the difficulty caused by its stopping at the os. To effect the re-inversion of the uterus in such cases by air-bags was, he thought, an impossibility. Pain was, no doubt, an important element, as affording a means of diagnosis, but too much stress should not be laid on that symptom. On the first examination he made of the tumour, a few days after the operation, he found that the mucous membrane of the surface of the inverted fundus had healed so perfectly that they could not suppose that any tumour had grown from that portion of it.

On the Prevention of Accidents arising from the Use of Pessaries. By J. RUTHERFOORD KIRKPATRICK, M.B., &c.

OF the different modes of treatment for displacement of the uterus, the use of pessaries is now most general, either with the hope of curing the displacement or of alleviating the sufferings and discomfort caused by these various flexions, versions, and prolapses of the womb.

It is not my intention in this paper to discuss the many symptoms, or

causes, or modes of treatment of these affections, or to enter very minutely into the merits or demerits of the innumerable kinds of pessaries which have been invented, used, and improved upon again and again.

It is my wish rather to investigate what are the accidents that may arise from the employment of these mechanical agents, and what are the best means to be adopted for their prevention. Many instances are recorded of very sad, and sometimes fatal, results following on their use—sometimes brought about by the total neglect on the part of the patient of the advice and instruction given to her at the time of their introduction; or it may be by neglect on the part of the physician in not giving proper directions or instructions to his patient; or, as sometimes does occur, from a physician making use of a pessary without acquainting his patient at all that he has done so. I pass by any reference to accidents arising from the use of intra-uterine stem pessaries, as I have had no experience myself of their employment. “These instruments are not popular with the great body of general practitioners, and by some of the most eminent specialists on diseases of women their use is altogether discountenanced, or restricted to a class of cases very limited indeed, as compared with those for which the inventors would have thought them fitting” (*Med. Times and Gaz.*, May 4, 1878). The vaginal pessaries most in use at the present time are the disc, the globular, the ring, Zuank’s and Hodge’s, or lever pessary, with its many modifications. The accidents arising from these are various—such as vaginitis (sometimes in a mild form, at times most intense), irritability of the bladder, complete retention of urine, constipation and engorgement of the lower end of the rectum. Besides these ill consequences, pelvi-peritonitis, peri-uterine inflammation, cellulitis, and metritis, have been induced; and in some cases, where the patient had recently been suffering from any of these latter forms of inflammation, the introduction of a pessary has brought about a fatal result. The cervix uteri has sometimes been incarcerated or even strangulated in the central opening of the disc, or within the old form of globular boxwood pessary, where that portion screwed into one end has been pulled away and the pessary has become turned round. The removal of a globe or disc pessary is often rendered very difficult, or next to impossible, in consequence of the orifice of the vagina having become much contracted. Under these circumstances it is sometimes necessary to break the pessary into fragments in order to accomplish its removal. If it happen to be made of vulcanite, the difficulty of this operation becomes very great, owing to the hardness and cohesion of the material out of which the pessary is made. Hodge’s pessary has been known to cut through the vaginal walls. I have not myself met with any instance of those sad accidents where the pessary has cut its way into the bladder or into the rectum, but many such deplorable cases are on record. A few years ago I had to remove from the vagina of an old woman a

large globular boxwood pessary, which was used to remedy a prolapse of the womb. This pessary had been placed in her vagina by Dr. Evory Kennedy when he was Master of the Rotunda Hospital, and had never been removed from that time till she came under my care, fully thirty-five years afterwards. Its removal, as may be supposed, was no easy or agreeable operation. The outlet of the vagina had become much contracted; the fœtor from it was such as I have never before or since experienced. Assisted by Dr. Tate, I succeeded in breaking the pessary into pieces with a strong bone-forceps and crotchet, and removed it bit by bit. This patient's vagina was in a state of intense inflammation and excoriation, for the relief of which she had sought advice, the commencement of her uneasiness being of rather recent date. I have only to add that she recovered without any bad symptoms or suffering, which one might suppose would follow after the contact of such putrid matter with an excoriated and inflamed mucous membrane. She had no return of prolapse.

In 1863 Dr. Kidd exhibited, at a meeting of this Society, a pessary which he had removed from a patient, seventy years of age, which had remained in her vagina for thirteen years. The pessary was formed of a mass of tow covered with some black composition. This woman stated that, notwithstanding the directions she got to apply at the hospital to have the pessary removed occasionally to have it cleaned, she had not done so, and had almost forgotten it till a fortnight previous, when she began to suffer pain in the back and from vaginal discharge. Dr. Kidd also mentioned that on another occasion he removed a large globular boxwood pessary which had remained in the vagina twenty years.

These cases are examples of patients neglecting the instructions given to them when the pessary was first used—this neglect, fortunately for themselves, leading to no worse sufferings than a bad attack of excoriation of the vagina, notwithstanding the length of time that the pessary had remained in the vagina. Much more serious consequences than these may ensue.

In Vol. XVII. of the London Obstetrical Society's Transactions, a case of recto-vaginal fistula is reported by Mr. Churton, caused by a Zuank's pessary retained for two years in the vagina. Although this woman's sufferings began about nine months after the introduction of the pessary, she neglected seeking relief until fæces had been passing from her vagina for three weeks.

Several other instances of both recto-vaginal and vesico-vaginal fistula were related by Fellows, during the subsequent discussion of this case, induced by different forms of pessary retained in the vagina for periods of time varying from fourteen months to eleven years. Dr. Wiltshire also remarked that in gouty women there was a great intolerance of pessaries, incrustations rapidly ensuing in such persons.

In Vol. XVIII. of the Transactions of the London Obstetrical Society, Dr. Gervis is reported to have exhibited a pessary which had remained over fifteen years in a woman's vagina. This patient had suffered from vaginal discharge, tinged with blood and highly offensive, for months. She could neither stand nor walk, and had cramps in the legs, but no pain either in passing water or in the action of the bowels. The pessary, which was a large round metallic one, was embedded in the walls of the vagina; it was removed with a bone-forceps, and the patient recovered perfectly.

In Vol. XIX. of the Transactions of the London Obstetrical Society there is a case reported by Dr. Galabin, and a Zuank's pessary exhibited by him, which had been removed from a woman fifty-eight years of age. This pessary, which had been introduced six years before, was found deep in the vagina, embedded in phosphatic calculi, the base of the bladder being destroyed, and the bladder, vagina, and rectum thrown into one common cloaca. In the discussion which took place on this case several speakers condemned Zuank's pessary as most objectionable. Dr. Godson, however, stated that during the last eight years he had inserted hundreds of them, and had never seen any evil results from wearing those that he had introduced. If the instrument was not removed every night it was undoubtedly liable to damage the soft parts. He always gave the patient printed instructions how to manage it, and had every reason to believe that these were carried out.

Mr. Christopher Heath, in a lecture on Diseases of the Rectum, published in *The Lancet* for 1873, says:—"Pessaries, used to rectify displacements of the womb, often cause much harm to the bowel, by their pressure causing engorgement of the lower end of the rectum." He also relates an example of the ill effects of a Zuank's pessary, one of which he had lately removed; it had caused an incurably large recto-vagina fistula.

We will now examine what rules are laid down in the standard works on diseases of women, for the avoidance of accidents during the use of vaginal pessaries.

Denman, in his Introduction to "Midwifery," says:—"Much dexterity and judgment also are required in the introduction of pessaries of any kind, for if they are too small they will not remain in the vagina, and if too large they will inflame and irritate the parts, mechanically causing strangury, obstinate constipation, and other painful symptoms." Further on he states, "Pessaries once fairly introduced may often be worn for many years without any or very little inconvenience; but sometimes, from the long continuance of a common one, or from the entanglement and strangulation of the os uteri, within the opening at the centre (which ought always to be very small), there has been much difficulty in withdrawing it when necessary."

In his work on "Diseases of Women," West says:—"It can scarcely be necessary to say much with reference to the manner of introducing pessaries or the precautions to be observed by those who wear them.

"Even in cases that most require their employment it is always pre-supposed that they are not used so long as any considerable tenderness of the parts exists, or as long as there are any remains of inflammation or of considerable congestion. Whatever kind of pessary is used, but especially when the disc pessary is employed, we should not leave our patient after its introduction until she has walked two or three times across the room, and thus ascertained that the instrument still remains in its proper position.

"No pessary should be allowed to remain for many weeks in the vagina, whatever may be the precautions used by frequent employment of vaginal injections to prevent the deposit of the secretions upon it. One of the great advantages of the globular pessary, and also of Zuank's instrument, consists in the possibility of its being removed by the patient herself at night, and replaced before she rises in the morning, by which means not only can it be kept scrupulously clean, but the vagina can be washed out by the copious use of water or of some astringent lotion twice in the twenty-four hours.

"Cases of most serious mischief, arising from the neglect of this precaution are on record, in which inflammation and ulceration of the vagina have been produced, or the pessary has even made its way into the bladder, thus entailing on the patient all the miseries of vesico-vaginal fistula."

M'Clintock, in his "Clinical Memoirs on Diseases of Women," says:—"Except on a few rare occasions, I have never seen any unpleasant consequences from their use. Using too large a pessary (a globular boxwood one) has two or three times been followed by some symptoms of uterine or pelvic inflammation, which subsided in a few days under mild treatment and the removal of the instrument. In about six or eight instances I have seen slight vaginitis or abrasions of the vagina induced by the prolonged detention of the pessary." In procidentia of the womb, with ulceration of the vagina or os uteri, he advises not to introduce a pessary until the patient had undergone preparative treatment during some days—the womb being returned to its proper place, the patient kept in bed, &c., &c.—with the expectation of the ulcer healing; but he gives an instance where, notwithstanding the presence of an ulcer fully two inches square, which would not heal in consequence of the impossibility of keeping the womb up, a globe pessary was introduced, which succeeded in keeping the womb up. As a measure of precaution it was taken out every three or four days and a careful examination made, to see that it was doing no mischief. The ulcer rapidly healed up. He advises that where a patient cannot herself remove the pessary, she should be peremptorily enjoined

to return in a couple of months—or sooner, should it cause the least pain or uneasiness—to have it taken out for a day or two and cleaned. The tape for the withdrawing of the round or oval pessary he directs should always be fastened to the opposite end of the pessary to that into which the circular piece of wood screws, for fear the movable piece should be pulled out, and so give rise to the accident of which he gives an instance. An unmarried woman, aged twenty-six, who had suffered from prolapse of the womb since she was eighteen, in trying to withdraw a globe pessary (as she was in the habit of doing herself from time to time by means of the string), pulled away the round button, leaving the instrument behind. Six weeks afterwards it began to cause her so much pain and annoyance as to compel her to seek relief. On examination, the pessary was felt, and he (Dr. M'Clintock) was at much pains to extract it with the aid of one blade of the midwifery forceps. He succeeded in getting it beyond the vulva, and then found that the cervix uteri was incarcerated within the pessary, and before they could be separated some forcible manipulation had to be used. The pessary had caused considerable abrasion of the vagina and os uteri.

Gaillard Thomas, of New York, in his work on “Diseases of Women,” says of pessaries:—“None should be used which distend the vagina, and those employed should be worn without any sense of discomfort, should be kept clean by injection with astringent fluid every night or every night and morning, and should be examined at intervals by the physician, to make sure that it is not cutting into the tissues; and at intervals not exceeding two months it should be removed, examined, and reintroduced. Their injurious effects,” he says, “I would attribute, not to the instruments themselves, but to the improper manner in which they are sometimes used and the carelessness with which they are allowed to remain *in situ* without observation. Pessaries should be carefully watched, for they sometimes create cellulitis, peritonitis, and vesico-, recto-, and utero-vagina fistula.” He relates a case where Professor Sayre dilated the cervical canal, and extracted a globe pessary which had migrated from the vagina into the uterus and been retained there for a length of time.

Marion Sims, in his work on “Uterine Surgery,” after mentioning various examples of serious results he has seen from the use of different kinds of pessaries, says:—“We will often make mistakes—1st. In resorting to pessaries when there is metritic inflammation in some form. 2nd. In selecting an inappropriate instrument. 3rd. In making it too large; sometimes too small. 4th. In allowing it to remain too long without removal. After we succeed in getting the pessary to fit accurately we should never send our patient off till she is taught to remove and replace it with the facility that she would put on or pull off an old slipper. A pessary is a thing to be worn like an eyeglass, only when awake. If we

observe these rules," he adds, "there would be none of the accidents alluded to above to damage their reputation for usefulness."

In Ziemssen's "Cyclopædia of the Practice of Medicine," Schroeder, speaking of the use of pessaries, says:—"This variety (Mayer's India-rubber ring) is less irritating than others, provided it is removed and cleaned from time to time—say every four weeks—and the vagina syringed. If they are left in position for a long time uninterruptedly, these, as well as every other kind of pessary, may produce the worst possible results." He had seen incisions into the soft parts made by the ring, into which one could lay the entire first joint of the finger. "Pessaries may also produce acute catarrhal inflammation of the vagina. A good, well-fitting pessary increases the secretion of the mucous membrane but very little, especially if the decomposition of this secretion is prevented by frequent cleansing. When pessaries press continuously upon one spot for a long time, they gradually erode the mucous membrane, and may cut deep furrows into the subjacent cellular tissue. Granulations growing through the opening of the pessary may even completely embed one side of the instrument."

Bernutz and Goupil, speaking of pessaries used against uterine deviations, where there has been uterine or peri-uterine inflammation, lay down this rule: The acute stage must have passed over—they must be easy and painless where applied, for we have seen their untimely application in a case of chronic pelvi-peritonitis bring on a fatal relapse.

Now, what rules of practice can we deduce from an analysis of the foregoing quotations.

No very precise rule has been laid down as to the length of time that a pessary should be allowed to remain in the vagina without removal. I think that where we use a pessary in the globular or oval or disc form, where the vaginal syringe cannot sufficiently keep the instrument clean, four weeks should be the limit of time that it should be permitted to remain without removal for the purpose of cleaning the instrument and examining the condition of the vagina and cervix uteri.

Those pessaries which are made of hard India-rubber, such as the different modifications of Hodge's, not being so liable to absorb moisture, keep clean much longer, particularly if the patient is careful to observe the rule to syringe the vagina, night and morning, with abundance of tepid or cold water. But even when these kind are worn we should direct our patient not to allow more than six weeks or two months to pass without an examination, either for the removal of the pessary or for its adjustment. I lately removed a Hodge's pessary, made of vulcanite, which had been introduced not more than four months previously. It was much coated over with phosphatic deposit, in such a manner as to be highly irritating to the vaginal mucous membrane, and which deposit could not be removed by any amount of washing.

We may instruct our patients to remove a pessary of any form if she feels pain, or inconvenience, or distress, while wearing it; but I would not like to trust a patient to reintroduce for herself a Hodge pessary, or indeed any form, unless, perhaps, a globular one for prolapse of the womb.

The strict observance of the following rules, I believe, will tend much to the safety and comfort of our patients, and prove the surest safeguard in preventing all the accidents arising from the use of pessaries:—

1st. In no case should a physician use a pessary without telling his patient that he has done so.

2nd. That, inasmuch as a pessary should give rise to no pain or uneasiness whatever after its introduction, the patient should immediately come back in the event of any such symptom occurring.

3rd. She should return to have the instrument examined or removed not later than six weeks or two months.

4th. That, in the event of any sensation of heat, pruritus, or irritation of the vagina, or the occurrence of any discharge, coloured or otherwise, or any offensive smell, she should at once present herself to the doctor for examination.

5th. The daily use of a vaginal injection of tepid water, or of some mildly astringent wash, will have the most beneficial effect in arresting the first symptoms of vaginal irritation. If the patient be wearing a Hodge pessary she should be warned to use gentleness in introducing the pipe of the syringe, lest the instrument might be displaced. A patient need not be afraid of using an injection of simple warm water, even during the menstrual flow. This conduces very much to her comfort at the time, and the retention of any of the catamenial fluid is very apt to prove a source of irritation.

6th. If the vulva is much dilated from procidentia of the womb, it is well to keep the patient in the horizontal position for some days, whereby the womb will be kept up, and the vaginal orifice have time to contract before the introduction of the pessary; for, although the introduction of a disc or globe pessary may be accomplished with great facility, its removal afterwards may become a matter of great difficulty, owing to the contraction of the vaginal orifice consequent upon the womb being kept *in situ*, and no longer distending the parts.

The PRESIDENT.—Did I understand you to say that the pessary migrated into the uterus?

DR. KIRKPATRICK.—So it was stated. Professor Sayre removed it from the interior of the uterus.

DR. M'CLINTOCK said Dr. Kirkpatrick had alluded to an accident that might occasionally arise from the use of pessaries—namely, the contraction of the os vaginæ subsequent to the introduction of the pessary;

and he had said that before introducing the pessary it would be well to keep the patient in bed for some time. A case illustrating this had recently come under his observation. An elderly unmarried lady, a person of nearly sixty years of age, of active habits and very strong muscular frame, came to him, complaining of a slight prolapsus of the uterus which had come to the vulva, annoying her excessively and interfering with her locomotion very much. He introduced a small disc pessary made of vulcanite, enjoining the use of a syringe every night, and told her if the pessary gave her any annoyance, or if there was any discharge, to come back to him. At the end of three years she came to him, and said that in the interval she had had the most perfect comfort with the pessary, and that she would not have come back but for his injunction to do so in the event of there being any discharge. He made an examination and found that there was no heat at the vulva, but he thought it would be well to take out the pessary for a few days in order that if there should be any irritation or inflammation higher up it might heal. On attempting to remove the pessary, all the force that he could use would not get it out, and caused such pain that he desisted. A few months ago a tall, fine-looking woman, of about sixty years of age, presented herself to him, complaining of a slight degree of pruritus with some discharge, which was occasionally red. She told him that she had ceased to be unwell for ten years previously, but that about seven years before a practitioner introduced a pessary for her in consequence of a retroversion of the womb, and that it had never been removed. He made an examination, and found a pretty large Hodge's pessary in the vagina. He succeeded in grasping the pubic end of it with his finger, but no effort consistent with safety could move it. Making further examination, he found a band of strong tissue passing through the posterior end of the pessary; in fact, he discovered that the posterior part of the os uteri had contracted a firm adhesion to the posterior wall of the vagina. He succeeded by scratching with his nail in tearing the tissue, and by a little traction was able to get the instrument away. On making an examination next day with a speculum he found a very considerable raw surface on the posterior wall of the cervix uteri and also on the posterior wall of the vagina. However, under simple treatment these healed up, and the lady was cured of her retroversion. It was four or five months since this happened; he had seen her several times since; and she had had no return of the symptoms, but entertained a considerable dislike to pessaries. They had before them the instrument he removed from the lady, which was of white metal, of the description used by Barnes and other leading men in London. Rightly or wrongly, however, he entertained a strong objection to metallic pessaries.

DR. MACAN said the effects of the ordinary Hodge's pessary were very doubtful. He had lately a case of retroversion of the uterus which

changed to ante-version without any pessary at all. He believed that the effect of astringents to the neck of the uterus was, to a certain extent, to cause the ligaments to tighten and to draw the uterus forward.

New Form for Note-taking in Obstetric Practice.

DR. ROE, in the absence of Dr. Kidd, exhibited a Table, entitled, "A Ready Method of Obstetric Note-taking," proposed by Dr. John W. Martin. It consists of a sheet of paper having space for name, address, &c., of patient, and on it printed almost every contingency that might arise in a labour, and the different plans of treatment that would be generally adopted. He proposes to tick off each as it occurs in a case, and fill the table for future reference.

TRANSPLANTATION OF A DOG'S CORNEA TO THE HUMAN EYE.

M. SCHOELER relates the case of a man, aged twenty, one of whose eyes was atrophied, while the other had just lost the entire cornea through its prolonged ulceration. The iris, covered with granulations, was laid entirely bare, the lens had dropped out. The patient had merely luminous sensations. M. Schoeler operated by cutting a large upper conjunctival flap, capable of covering the whole extent of the cornea; then below, a small flap intended to be united by points of suture to the upper flap that was turned down, the epithelial surface of both flaps being turned back against the surface of the globe. By means of a trephine he removed from the eye of a chloroformed dog a circular portion of the cornea, about $9\frac{1}{2}$ millimetres in diameter. This cornea being applied to the vacant space in the human eye, he brought down in front of it the large conjunctival flap, which he united by catgut sutures to the small flap. The transplanted cornea was thus held in position and protected by the conjunctival flaps. At the end of three days the sutures fell out, the conjunctival flap was adhering to the transplanted cornea, and the latter to the margin of the sclerotic. There was an anterior chamber visible where the conjunctiva was deficient. But on the following days the cornea gave trouble, and finally became of a milky tint—an ulcer appeared. By degrees vessels found their way into the periphery of the cornea and reached its centre. After the sixth week the conjunctival flap was detached. Eight days afterwards the cornea was flat, very opaque in the centre, but translucent at the periphery so as to let the iris be seen. The vision is, however, very slight: the movement of the hand can be distinguished at a distance of half a foot from the eye.—*La Revue Médicale.*

S. W.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

President—EDWARD HAMILTON, M.D.

Secretary—E. H. BENNETT, M.D.

Fracture of the Patella United by Bone.—DR. WHEELER said: The specimen which I lay before you is one of undoubted bony union of a fractured patella. The bone has been macerated and a section made of it. The following is the history of the case:—

William Fitzgerald, by occupation a milkman's driver, residing at 11, Church-lane, was admitted into the City of Dublin Hospital, under my care, on the 13th September, 1875, suffering from a transverse fracture of his left patella, which occurred when getting down from his milk-cart. There was considerable effusion into the joint, and the fragments were separated more than two inches. I placed him on my own apparatus for such injuries which I have shown and described at other Societies, and applied ice-bags over the joint; in eleven days the effusion was got down sufficiently to allow of the fragments being brought into apposition, which was effected by means of the pads attached to the apparatus. He was so kept from the 24th September till 5th November, when a starch bandage was applied, and he was allowed to get up and walk about the ward. He was discharged from hospital on the 13th of December, the fragments being firmly united. There have been so many cases of close union, but not bony union, that I watched this case carefully to ascertain whether the uniting medium would stretch or not; there was every provocation for its doing so, because the man had to get up and down from his cart about 240 times a week. In the year 1877 this patient consulted me for a cough. Examination proved he had incipient phthisis. I admitted him into hospital, where my colleague, Dr. Hawtrey Benson, saw him with me. After a time he thought change of air would be beneficial to him, and through the kindness of Dr. Gordon he was admitted into the Whitworth Hospital, where he died early this year. By the courtesy of Mr. Martin, house surgeon to the above institution, I have been able to produce his two patellæ here—the sound and broken one. In the latter the transverse line of fracture is visible on the posterior aspect (Fig. 1, *a a*), where there is a slight bony ledge, which did not in any way interfere with the movements of the patella or the cartilage which covered it over completely. On the anterior aspect of the bone there is no visible trace of fracture save a slight depression at the inner side; on the lower fragment and

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Fig. 1.

Fig. 2.

Fig. 3.

outer facet there are signs of an incomplete longitudinal fracture (Fig. 1, *b b*). The bone was macerated and the section made by Professor Macalister. There was no diminution of the quadriceps extensor muscle, the patella moved freely over the femur, its motions (normal) being in no way impaired; there was no lessening in the circumference of the limb; he suffered from spasms of the quadriceps extensor, which were considerable when the upper fragment was pressed on. These facts are contrary to the experience of a writer in the "Medico-Chirurgical Transactions." The exact distance between the fragments was two and three-eighth inches before adjustment. I have not the slightest hesitation in saying that I believe the causes of the bony union were the perfect position the fragments were kept in by my own splint, and the correct traction made by it, by which no tilting of the fragments can occur. If there is difficulty in getting down the upper fragment to meet the lower it is competent to draw upon the inferior fragment, which Malgaigne erroneously states is pulled down by the elasticity of the ligamentum patellæ. I have examined all the specimens at the College of Surgeons—some of them were stated to be bony unions—and in all but one (Dr. Kirby's) the fragments are distinctly separated. In this case (Dr. Kirby's) there is only an example of partial bony union. I am aware that in the case described by Gulliver it was considered that the bony union was in consequence of the fibrous capsule not being torn.—*November 30, 1878.*

THE PROXIMATE CAUSE OF ORCHITIS.

M. DESPRES read before the Académie de Médecine a communication on this subject. The following are his conclusions:—1. Orchitis is due to the retention of spermatic fluid in the testicle. 2. The cause of the retention of the spermatic fluid is not always to be found in the same spot, but it is more than probable that the swelling of the mucous membrane of the ejaculatory ducts and of the vas deferens, or even congestion of the prostate or of the lining membrane of the urethra, are the ordinary causes of the retention. 3. The appearance of orchitis in gonorrhœa—from the tenth to the twentieth day—coincides with the functional activity of the testicle. The more active the state in which the organ is in the sooner will the orchitis show itself after the beginning of the urethritis. 4. Orchitis produced by a wound or by irritation of the urethra can be explained by a tumefaction of the wounded parts, and particularly by a tumefaction occurring at the level of the ejaculatory ducts and of the vesicula seminalis, and arresting the flow of the spermatic fluid into its natural reservoirs.—*La Revue Médicale.*

S. W.

TRANSACTIONS OF THE CORK MEDICO-CHIRURGICAL SOCIETY.

SESSION 1878-9.

President—DR. D. D. DONOVAN.

Secretary—DR. D. C. O'CONNOR, Jun.

Are Croup and Diphtheria the same Disease? Pathological Specimen exhibited by DR. CORBY.

GENTLEMEN,—I exhibit for your inspection this evening a pathological specimen, consisting of a larynx and trachea on which there is deposited a false membrane. Unfortunately, I have very little history of the case, as the patient died soon after admission to hospital. She was a little girl aged a year and two months. On admission I could distinctly see a deposit on the uvula and also on one or both tonsils. The mother stated that she had noticed the child “choking” during the past two or three days, but thought she was only suffering from a cold. During the night after coming into hospital the little patient died rather unexpectedly, and next morning I learned that her death was preceded by a good deal of dyspnoea and some convulsions. On making the autopsy I found a false membrane lining the uvula, and one covering a great part of the epiglottis like a glove, and then extending downwards into the trachea, where it became thinner; and farther down in this portion of the respiratory tract I found one or two detached portions lying loosely on the mucous membrane. So far I had before me an interesting pathological specimen; but then came the important question—what disease does it exemplify? Whether diphtheria or croup? Difficult as that query proves in life, in this instance I may say I found it equally difficult in death. For I know of no means by which you can decide on examining that specimen which of the two diseases it illustrates, or whether it illustrates both. This brings us to the important question—whether diphtheria and croup are really two distinct diseases, as believed by the majority of practitioners in this country, or whether they form one and the same disease, as held by the French school. Assuming, then, for a moment that croup and diphtheria represent two perfectly distinct diseases, how shall we diagnose the one from the other in life, and how shall we distinguish them in death? Flint, in giving the diagnosis of diphtheria, states that “the enlargement of the cervical glands is greater

in cases of diphtheria. Albuminuria is not of frequent occurrence in primary croup; the febrile movement and other general symptoms are more symptomatic of the local affection, and the affection is not followed by paralysis. Moreover, primary croup is a sporadic affection, whereas diphtheria prevails as an epidemic, and the latter does not, like the former, affect almost exclusively children." In addition to those diagnostic signs, I believe both Dr. Churchill and Professor Spence point out that the diphtheritic membrane might form on an abraded surface or on a mucous membrane away from the pharynx, and that such is not the case with reference to the croupous membrane, at least when the croup is primary. Furthermore, Dr. Churchill tells us that if we examine the throat shortly after the commencement of diphtheria we will find some redness and swelling of the tonsils. Whereas if in the precursory stage of croup, should such a stage exist, if we examine the pharynx we shall discover "no trace of disease." Those are all matters well worthy of consideration, but there is one statement in Flint, under the heading Anatomical Characters, which might, I think, help us materially in elucidating the question. Referring to the false membrane of diphtheria, he says:—"It is essentially identical as regards gross and microscopical characters with the false membrane in true or diphtheritic croup." As we have then but the one membrane, may we not assume that we have but the one disease, the symptoms of which are modified, and often considerably, by the position of the membrane, whether it be situated up high in the pharynx or down in the larynx. And now let us see if we cannot explain some of the supposed diagnostic differences on this hypothesis. We shall take them *seriatim* as I have mentioned them:—

(1.) "The enlargement of the cervical glands is greater in cases of diphtheria." Assuming now that we are dealing with only one disease, how can we account for the fact that the cervical glands are larger when that disease is in the pharynx than they would be were it in the larynx? The explanation is simple. The pharynx is supplied with lymphatics more plentifully than is the larynx, and a larger number of glands are nearer to the pharynx. Hence the morbid matter is taken up more readily from a membrane situated in the pharynx than it would be from one placed in the larynx; and hence the greater lymphatic enlargement referred to.

(2.) "Albuminuria is not of frequent occurrence in primary croup." Explanation: the albuminuria is a result of a pernicious change produced in the blood by the disease; but as, for reasons I will give further on, patients die much sooner when the membrane is in the larynx than they do when it is in the pharynx, there is not usually sufficient time for the production of the albuminuria as the result of the pernicious change in the blood.

(3.) "The febrile movement and other general symptoms are more symptomatic of croup." Explanation: the membrane causing tumefaction

of the epiglottis, and thereby interfering with respiration, will be more likely to produce feverish symptoms than would the same membrane lying on the back of the pharynx.

(4.) "Croup is not followed by paralysis." I think I can deny this statement on the authority of Niemeyer who, by means of the laryngoscope, found immobility of the glottis when it should open out for the purpose of admitting the inspired air into the lungs. This condition, too, of the glottis, combined with tumefaction and spasm, resulting from the deposit of the membrane in this neighbourhood, all combine to produce apnoea, and this accounts for death being likely to occur earlier than it would if the membrane were in the pharynx.

(5.) Flint alleges that primary croup is *sporadic*, whereas diphtheria prevails as an *epidemic*, but Dr. Churchill states emphatically that "croup prevailed epidemically at different times." He further states that diphtheria usually prevails as an epidemic, but also tells us that the crowding together of children in a close habitation *may give rise to it*. It is evident, then, that the disease, whether exhibiting itself in the pharynx or larynx, might prevail either sporadically or epidemically.

(6.) "Croup affects children almost exclusively, whereas diphtheria does not." Explanation: I am disposed to hold that the membrane usually commences below. Look into a child's throat. You see nothing whilst the membrane remains below, and you describe it as a case of croup. If the child should live long enough, and the membrane creep up, preceded by swelling and redness above it, you then change your diagnosis, and call the disease diphtheria. Taking this view of the disease, we can readily understand how, considering the minute size of the rima glottidis in a child, what between spasm, tumefaction, and immobility of the glottis, apnoea would have done its worst before the membrane would have extended up sufficiently high to attain to the title of diphtheritic; whilst in the adult, the chink of the glottis being much larger, there would, in many instances, be ample time for the extension of the membrane to the pharynx.

(7.) Dr. Churchill, I believe, considers that diphtheritic membrane might form on mucous membranes away from the pharynx, and that the croupous membrane is confined to the throat. But we have already had it from Dr. Flint that the membranes when seen *cannot be anatomically distinguished*. Dr. Churchill again tells us, and he must have given a good deal of consideration to such matters, that should diphtheria invade the larynx "the symptoms will be those of croup already described."

Taking now a general review of the whole subject, I feel very much disposed to agree with the French physicians in looking on diphtheria and croup as forming only one disease, the symptoms of which are affected according to the position or positions in which the characteristic

membrane may be placed, and I shall be most happy to have the opinions of the members of this Society on the above subject, which I consider of vital importance.

Notes on the Removal of a Penny which had been Several Days in the Oesophagus. By DR. CORBY.

A boy about twelve years of age, named Thomas Coleman, presented himself for treatment at the North Infirmary on the night of Friday, the 29th of last September. His mother stated that about six o'clock that evening he was playing with an infant, and held a penny between his teeth, and was showing it to the child. The latter struck the penny, and it disappeared in the young fellow's throat. He complained that he was choking, and the mother endeavoured to extract the coin by following it with her finger. She succeeded in touching it, but only with the effect of driving it further down. He then felt relieved, and said it was gone. However, she deemed it prudent to come to the Infirmary for the purpose of getting any other treatment that might be necessary. I saw him, and assuming that the foreign body was either in the stomach or intestines, directed him to take nothing but soft food, and gave him some opium powders, my desire being to check the peristaltic action, and form a bolus with the coin in the centre. Next morning the patient called again, and it occurred to me that it would be well to pass the probang, and accordingly did so. The moment the instrument was withdrawn he exclaimed, "Tis gone down," and I then directed him to continue the treatment as on the previous evening. The following Wednesday he called again. There was salivation since Sunday—a large quantity of fluid coming from his mouth, and staining his handkerchiefs puce; the tongue was blistered, and the gums appeared irritable. There was considerable dysphagia, but its extent varied with the substances swallowed. "Stirabout" used to hurt him, as also bread and milk, but not to the same extent. He found tea too rough to the throat, but soup or milk was not so troublesome. There was a change in his voice, which his mother attempted to describe by saying it was hoarse and old, as if he were ninety. He had pain near the right nipple, a little to the inner side, and shooting back below the right scapula. Bowels had not been opened since he swallowed the penny, and I now ordered him some salts and senna with a view to carrying off the coin, which I assumed to be contained in a bolus, as already described. Next day (Thursday) he called again, and said that owing to a dislike for salts and senna he had taken a pennyworth of jalap instead, and the bowels were moved, but without disturbing the coin. Considering all the symptoms, I thought I had better pass the probang once more, and on drawing it up along the oesophagus distinctly struck it against the penny. I then examined the probang, and found on the sponge some dark scaly matter, which I regarded as sulphide of copper

which had been brushed off as the instrument was being withdrawn. I again introduced it, carefully avoiding the obstruction, till I got below it, and then withdrew it quickly, tilting the sponge towards the penny. The penny was jerked up; and I now produce it for your inspection. It still is covered with sulphide of copper, produced by the combination of the copper with the sulphuretted hydrogen of the breath, and you may also notice the indentations of the teeth on the metal. Lest any of the sulphide might still remain in the stomach, I gave an emetic of ipecacuanha, and the fæces do not appear to have been discoloured. I have since learned that for days after the coin was removed he was drowsy and indisposed for mental exertion, but I have recently seen him, and he appears in his usual health. As the penny was in the œsophagus from Friday at six o'clock in the evening till about noon on the following Thursday, it is evident it was there very nearly six days. If those notes of the case will prove interesting to the Society, the writing of them will have afforded me much pleasure.

VOCAL PARALYSIS IN PHTHISIS.

IN the *Aerztliches Intelligenzblatt* Dr. Kittler observes that vocal paralysis occurring in the progress of phthisis may be of two kinds, either combined with lesions of the mucous membrane or with the mucous membrane intact. The most frequent sources of the former are catarrh and ulceration. Although inflammatory swelling of the mucous membrane is in itself sufficient to produce hoarseness, yet in these cases we must consider the impairment of the voice due rather to a paralysis of the laryngeal muscles in consequence of the serous infiltration of the muscular fibres, than to the catarrhal affection, which is often very slight. The paralysis is mostly double, and affects chiefly the muscles which close the glottis and the tensors. The site of ulceration when this is present has considerable influence on the form of paralysis; thus ulceration of the posterior walls impairs the action of the transverse arytenoid muscle. Paralysis of the muscles that open the glottis is rare in phthisis. There is not only serous infiltration of the muscular tissue in laryngeal phthisis, but, as Fränkel has shown, a destruction of the muscular transverse striæ, which are replaced by a fine-grained molecular mass. By infiltration there is also mechanical interference with the free action of the muscles, as well as, in some instances, doubtless, sufficient pressure in the nerve fibrillæ to impede muscular action.—*St. Louis Med. Journal.*

S. W.

COMMENTARIES ON DISEASES OF THE KIDNEYS.

PART I.

By ARTHUR WYNNE FOOT, M.D., Univ. Dubl.; Senior Physician, Meath Hospital and County of Dublin Infirmary; Fellow and Ex-censor, King and Queen's College of Physicians in Ireland; Diplomate in State Medicine, Trinity College, Dublin; Lecturer on Practice of Medicine in the Ledwich School of Medicine and Surgery; Fellow, Royal Geological Society of Ireland, &c.

[Continued from page 88, Vol. LXVII.]

THE DOCTRINE OF URÆMIA.—Uræmic symptoms: (a) acute; (b) chronic—(a) Acute characterised by epileptiform convulsions succeeded by coma or maniacal excitement; (b) Chronic, by apathy, somnolence, coma. Defects of sight included among the group of uræmic symptoms—amblyopia uræmica—amaurosis uræmica. Retinitis apoplectica not necessarily associated with uræmia. Uræmic coma; sometimes a difficulty in diagnosis. Asthma uræmica, description of—according to Hertz due to interstitial cedema. The theories of Uræmia.—First, or English theory (Wilson, 1833)—Frerich's theory (1851)—elaborated by Treitz—Criticism of all the theories of uræmia by Rommelaere. Investigations of Traube on the cause of uræmia—the physical hypothesis as opposed to the chemical. Cases in which uræmia most commonly occurs. Uræmic coma. Temperature in uræmia—itching in, exudation of urea on the skin. Proclivity to inflammatory affections in uræmia—their tendency to purulent infiltration—frequency of phlegmons and gangrene. Effects of renal disease on the general nutrition of the body—anaemia, emaciation, weakness. Dyspepsia, vomiting, diarrhoea, frequent symptoms. Causes of the *dyspepsia*. Causes of the *vomiting* of renal disease. *Diarrhoea* more rare than vomiting, due to a variety of causes. The *hæmorrhagic cachexia* in renal disease—petechiæ—epistaxis; hæmorrhagic symptoms most common in contracted kidney.

UNDER the name of *Uræmia* a series of symptoms have been ranked together which have been regarded as the direct result of blood poisoning with urinary matters, the consequence of the retention in the blood of the dross of the capillary interchanges—the nitrogenous substances and specific urinary constituents. The very various functional diseases of the nervous system which, in accordance with general custom, are regarded as symptoms of uræmia, sometimes pursue an acute, sometimes a chronic, course.

Acute uræmia manifests itself in the form of epileptic convulsions, which are often terribly violent, and are succeeded by coma, or, in some instances, by a condition of maniacal excitement. In most cases the convulsions succeed each other at short intervals, often before the patient can recover from the coma of the preceding seizure. Not infrequently

a series of such epileptic attacks terminate at last with death. Cases, however, occur in which patients afflicted with a chronic renal affection, who have had one or a few such attacks following acute uræmic poisoning, recover again completely, and live on for a long time, pursuing their usual avocations. Occasionally, epileptiform convulsions are the first symptoms to suggest an insidiously progressing renal disease, attacking an individual in apparently perfect health. But, in other cases, these acute uræmic fits are preceded by other striking symptoms of the complaint—such as dropsy, dyspepsia, or obstinate vomiting. In rarer instances the characteristic convulsions are ushered in by cramp-like twitchings of particular groups of muscles, lasting for some days, or by tetanic spasms, which at first do not disturb the intellectual faculties. But far more commonly the first epileptic seizure assails the patient quite on a sudden and without any of these premonitions.

In the *Chronic* forms of *Uræmia* convulsive phenomena may be entirely absent, or they may appear in the shape only of twitchings of certain groups of muscles. The uræmia may show itself only by increasing somnolence, apathy, or stupefaction, advancing at last to complete coma. But this mental obfuscation is very often preceded by dyspeptic difficulties, by stubborn and incessant vomiting; and the vomited matters in such cases quite often contain large quantities of carbonate of ammonia, which gives them an alkaline reaction, can be perceived by the smell, and admits of chemical recognition without any great difficulty. There is often in this form of uræmia a most tormenting itching of the skin, which drives the patient, though in a stupefied state, to continually scratch himself. Lastly, it may happen that a patient who has been lying some days in a state of complete coma, is seized by one or by repeated epileptic attacks before death closes the scene.

In uræmia, either of the acute or chronic form, delirium is rare, while coma is frequent; paralysis of the limbs is scarcely known, unless there be some anatomical lesion of the brain superadded, while convulsions are frequent. The uræmic symptoms in the chronic form generally begin insidiously with headache or vomiting, followed by heaviness, indifference, and somnolence. The most common of these symptoms is headache; a sense of heavy weight or compression over the forehead or vertex is complained of; sometimes the pain is obstinately fixed at the back of the neck or behind the orbits. Uræmic convulsions are of the epileptic type. They are accompanied with complete insensibility, rolling of the eyes, biting of the tongue, and foaming at the mouth. The paroxysms commonly leave the patient deeply comatose. Uræmic coma may either creep on very gradually, passing on, in the course of two or three days, into complete stupor, or may culminate quickly—the patient falling down as if in apoplexy, perhaps while walking in the street or occupied with his usual avocations. Cases of this class, where there is no anasarca and

the previous state of the urine is unknown, are very liable to be confounded with apoplexy (and, in fact, have been described as serous apoplexy) or narcotic poisoning.

Among the group of uræmic symptoms are reckoned certain defects of sight which possess great clinical importance. One of these is a dimness of vision—*amblyopia uræmica*—which comes and goes, objects appearing as if veiled in mist. There is also the *amaurosis uræmica*—a sudden and complete blindness, which is usually temporary, and, as a rule, disappears as suddenly as it comes. This is to be distinguished from the disturbances of vision which so often occur in the course of chronic renal disease, and which are due to grave structural alterations of the retina, and are permanent in character. This latter affection—the hæmorrhagic blindness, *retinitis apoplectica*—is in no sense uræmic, though not uncommon in Bright's disease. It has been shown by von Graefe to be due to rupture of the retinal vessels, and its production is probably due to the hypertrophy of the left ventricle which so commonly accompanies a contracting kidney and the increased tension in the arterial system consequent thereon. It is believed that the pathological alteration in the eyes of those who, while suffering from renal disease, become suddenly amaurotic, consists in simple cedema of the retina.

Amongst uræmic symptoms are also to be reckoned those asthmatic attacks which sometimes assail the subjects of chronic renal affections a long while before their death. These attacks of asthma come on in paroxysms, with intervals of complete freedom, and are more frequent at night-time. This hæmic or uræmic asthma impresses a bystander like the breathlessness following over-exertion in health; the respiration is not seriously out of proportion with the pulse, and usually the subjective sensation of want of breath is, comparatively speaking, little felt; the patient breathes like a man out of breath from running (noisily), says he feels short-breathed, but does not look as if suffering therefrom. Pathologists are not agreed in ascribing this form of renal dyspnoea to irritation of the respiratory centre from poisoned blood. According to Hertz^a this condition is nothing more nor less than an *interstitial* cedema of the lungs with infiltration and swelling of the mucous membrane of the terminal bronchi. He fully agrees with Rosenstein in believing the so-called *uræmic asthma* to be due to an cedematous swelling of the bronchial mucous membrane, and not to the effect of the blood-poisoning upon the nerves governing respiration. He (Hertz) has hitherto only observed this cedema in persons in whom, during their illness, he was able to diagnose a primary cirrhosis of the kidneys, and subsequently verify it by *post mortem* examination; and he remarks that it is certainly surprising that cedema of the lungs is met with just in those patients who, while having the contracted kidneys, do not suffer from extensive

^a Cyclop. Med. Edited by Von Ziemssen. Vol. V. P. 279.

œdema of the surface or from fluid accumulations in the large internal cavities of the body, until death is near at hand.

The most strenuous efforts, made by quite a host of able inquirers, to discover the essential nature of *uræmia*, have only led to very conflicting views upon the subject, and the *doctrine of uræmia* has been subject to many fluctuations of opinion even in very recent times. In order to get possession of the more important of these opinions, it will be necessary to review very briefly some of the principal theories which have been entertained on this matter. The relation of the neuroses which are comprehended in the term *uræmia* to renal disease was clearly recognised by Bright, and the general opinion of that time was that the blood was contaminated with urinary elements, and so exercised a poisonous effect upon the nervous system. This idea was chiefly fostered by the proofs—first furnished by English observers—of an accumulation of urea in the blood of many patients affected with kidney disease. Babbington found, during life, 1·5 per cent. of urea in the blood-serum of a woman, twenty-four years of age, who was dropsical, had albuminous urine, and finally died with epileptic convulsions—the autopsy showing her to have had granular kidneys; and he remarked on it that this woman's blood-serum was as rich in urea as her urine. The promulgation of this, the first, or English, theory of *uræmia*, was made by Dr. Wilson, of St. George's Hospital, in a communication "On Fits and Sudden Death in connexion with Diseased Kidney."^a Dr. Wilson attributed the symptoms to diminished albumen and excess of urea in the blood—the discovery of Bostock and Christison. This theory soon came to be objected to on the ground that large quantities of urea might be present in the blood without any special nerve symptoms resulting therefrom. Owen Rees observed a case in which, while one kidney was altogether absent, the ureter of the other, enlarged by compensative hypertrophy, had been blocked up by a calculus. The consequence was a total suppression of urine. Although the patient had remained in complete possession of all his faculties up to the last moment, still O. Rees found more urea in the blood drawn from him during life than had ever been shown to be present in the urine of any case of Bright's disease with which he was acquainted. Furthermore, experiments made upon animals with injections of filtered urine and with solutions of urea have failed to induce fits, or conditions at all like *uræmia*. It may be taken as established, from many clinical observations, that the overloading of the blood with urea is certainly not, in every instance, the cause of the *uræmic* symptoms.

In 1851, Frerichs adopted the urea theory in a modified form. His proposition was that the urea in the blood, under the influence of some special ferment, is decomposed into ammonium carbonate, and that this was the pernicious principle which produced the functional disorders.

^a Medical Gazette, 1833. Vol. XI. P. 177.

Henceforth the term ammoniæmia came to be used by many instead of uræmia. Treitz perfected this theory of Frerichs by submitting that the urea retained in the blood and passing into all its secretions appears most generally and in the largest quantity on the mucous surface of the intestines. This urea, however, is regularly decomposed into ammonium carbonate by the intestinal fluids, especially the gastro-intestinal mucus, and the salt is then reabsorbed and carried back into the blood. In the course of time, however, objections from various quarters were raised against the doctrine of uræmia as propounded by Frerichs and elaborated by Treitz. The instances became constantly more numerous in which, notwithstanding the most careful analyses, no ammonia could be discovered either in the expired breath or in the blood of uræmic persons, even where uræmic coma and repeated epileptic convulsions were present, and where the face was covered with crystals of urea left behind by the evaporation of the sweat. A severe blow to the tottering theory was also given by Schottin, who called attention to the fact that ammonium carbonate can be found in the breath of any person in whose mouth the natural secretions collect and pass into decomposition—as, for example, in typhoid states, pyæmia, and other cases. From the observations which have been made up to the present, the only conclusion which we can accept is, that there are cases of uræmia in which ammonia is contained in the blood, and others of apparently the same nature in which chemical analysis has hitherto entirely failed to detect a trace of that substance, so that the theory of Frerichs can only be the correct one in, at most, a few cases.

Recent experiments of Oppler, Hoppe-Seyler, and others, indicate that uræmic manifestations depend mainly and essentially on the accumulation in the blood and tissues of those primary products of tissue metamorphosis (creatin, creatinin, leucin, and other extractives) which, in a later stage of histolysis, are converted into urea and uric acid, and excreted as such by normal kidneys. Zaleski concludes, from observations made in Hoppe-Seyler's laboratory on animals—such as snakes and birds, which do not in health produce urea, but stop short at the formation of ammonium urate—that Frerichs' theory of ammonia poisoning must be given up, and that the urea theory cannot be maintained, since uræmia occurs in animals that never form urea. Schottin found in uræmia the proportion of extractive matter to albumen in the blood to be increased from 5 : 100 to 40 : 100, and Hoppe found the creatin to be five times more than in healthy blood.

Dr. W. Rommelaere, Fellow of the Royal Society of Medicine and Natural Philosophy in Brussels, has subjected to a critical examination all the theories which from time to time have been proposed regarding the essential causes of the uræmic symptoms, and concludes that we can no longer accuse any one substance that may occur in the blood after

suppression of the urinary excretion, of being alone and by itself the cause of the nervous symptoms. The suppression of the urinary excretion prevents the further elaboration of albuminoid substances throughout the entire organism, if nature does not provide some other vicarious mode of supplementing this excretion. Urea is the last step or stage of the metamorphosis of these albuminoid substances, and when urea accumulates in the blood it changes the character of this fluid, and consequently the vital (nutritive) processes in various parts of the body. The entire work of transforming the albuminoid substances is arrested, so that we have to deal with the retention in the system, not merely of urea, but also of nitrogenous substances in all the different stages of oxidation through which they pass within the organism. It may be accepted as invariable that the maladies which entail uræmic disturbances are characterised at the time when the uræmic complications break out by the presence, in the tissues of the body, of an excessive amount of nitrogenous excretory materials. In suppression of the urinary excretion it is not one single element like urea or uric acid, creatin or creatinin, the extractives, or urochrom (pigmentary matter), which does the harm. It is the mass together.

It was the absence of anatomical lesions in the brains of persons who die of uræmic coma and convulsions which had constrained pathologists to invoke the aid of chemistry to explain the phenomena, and we have seen how ineffectual their efforts to do so have been up to the present. Traube, however, has instituted investigations in quite a different direction, and seeks to find out the essential cause of uræmia in local lesions of the central organs of the nervous system. He connects the symptoms with œdema and anæmia of the brain, the result of two factors: one is the thinning of the blood serum, which is so customary in renal disease; the other is hypertrophy of the left ventricle of the heart, by means of which an abnormal excess of pressure throughout the arterial system is effected at the same time that the blood serum is impoverished. Traube's hypothesis then is a physical one as opposed to the chemical hypotheses which have been just alluded to, and is as follows:—If from any cause the liquefaction of the serum or the tension of the arteries suddenly increases, œdema and anæmia of the brain follow, and, in consequence thereof, the symptoms of uræmia are produced. If only the cerebrum becomes œdematous and anæmic, simple coma arises; but if at the same time the middle brain (thalami and corpora striata) also becomes anæmic, convulsions occur; if only the latter becomes anæmic, convulsions merely are produced. Traube's theory has had in its turn to bear a trying examination, for which it does not seem to be in all cases equal; for example, in the post-cholera uræmia both the conditions laid down by him as requisite for the production of uræmia—namely, a watery blood and an elevated arterial blood pressure—are absent as a matter of certainty. Yet it is

equally certain that these attacks fairly merit the title of uræmic. They come on after long-continued complete suppression of urine, and are not infrequently accompanied by secretion of urea upon the surface of the skin. Further doubts as to the correctness of Traube's theory of œdema of the brain, or as to its applicability to every case of uræmia, are aroused by the fact that uræmic attacks do not by any means occur chiefly in those cases of renal disease which are characterised by dropsy. Indeed, so long as the dropsical swellings are still increasing, uræmic symptoms are, comparatively speaking, rarely observed. It is, indeed, a striking fact that uræmic symptoms often occur in persons suffering from renal disease who never, either at the time of the attack or previously, have been affected with anasarca or dropsical accumulations in the serous cavities. It seems that uræmia is both absolutely and relatively more common among the very cases of renal disease which are not dropsical, and in persons, therefore, who are but little hindered from following their ordinary avocations.

It has been gathered from clinical observations that the outbreak of uræmic convulsions is generally preceded by a diminution in the excretion of urine, and especially of the urea, to a figure far below the ordinary mean average, but it has not been explained why it is that uræmic symptoms are not forthcoming in cases which, with the daily excretion of urine and urea reduced to quite as low an ebb, advance to a fatal issue without the occurrence of convulsions or protracted coma. It is also a remarkable fact that uræmic convulsions come on in paroxysms, with intervals of perfect remission between them, the exciting cause remaining in undiminished force. The coma following a uræmic convulsion is ordinarily more protracted than that which succeeds the uncomplicated epileptic convulsion, because the former are usually more violent than the latter, and follow each other with so much greater rapidity that the patients sometimes do not recover their senses at all between the fits. Bartels has observed in every case of uræmic convulsion which he has seen, with the exception of one single instance, that the temperature of the body, taken directly after the first attack, has been considerably elevated above the normal. During uræmic coma the temperature may fall far below the normal, even quite a long time before death. Uræmia is often associated with an itching of the skin, which compels the patient to scratch himself incessantly, even while lying in a state of unconsciousness. Bartels has noticed this symptom oftener in cases of chronic uræmia where there have been no convulsions, and in such especially as exhaled a urinous odour from their persons. In uræmia crystals of urea are sometimes left on the skin upon evaporation of the perspiration in which it was held in solution. This occurs especially on the face, and may even give to the beard an appearance as if it were frosted over.

Among the results of the contamination of the blood by urinary matters retained in it is to be included a marked proclivity to inflammatory affections. So common are they in renal disease that, according to some, these inflammations are the immediate cause of death in the larger proportion of cases of Bright's disease, so-called; and they probably do lead to death, in the various forms of diffuse nephritis, more frequently than do dropsy, uræmia, and apoplexy reckoned together. The peculiarity of these processes of inflammation in renal disease is the tendency which they manifest towards purulent infiltration—the purulent infiltrations showing themselves most commonly as phlegmons of the subcutaneous connective tissue. Then, again, erysipelatous inflammations of the skin are often seen occurring in renal cases, and, next to these, perhaps, soft infiltrations of the lungs are the most common complications. Death by gangrene is the ordinary issue of the phlegmonous inflammations.

The general nutrition of the body is seriously influenced by renal disease in a variety of ways. The loss of albumen entails serious consequences upon the organism, and it is a matter of clinical experience that renal diseases produce their injurious effects upon the general nutrition—namely, anæmia, emaciation, and weakness—all the more quickly in proportion to the abundance of the loss of albumen which the organism experiences in consequence of the albuminuria which accompanies such affections. The waste of muscle and of subcutaneous fat which is commensurate with the waste of albumen that drains away in the urine, is apt to escape observation owing to the presence of dropsical swelling of the subcutaneous tissue. The wasting, however, becomes apparent at once, when, from profuse sweating, colliquative diarrhœa, or other similar evacuation, a sudden absorption of the dropsy takes place.

Dyspepsia, vomiting, and diarrhœa, are such frequent and ordinary symptoms of renal disease, that the mere fact of the repeated concurrence of these functional disorders of the digestive apparatus proves some causal relation between it and them. In some cases the *dyspepsia* appears to be the result of the extreme anæmia, provoked by renal disease, and, like that produced by all other anæmic states, is to be referred to an insufficiency of the secretion from the peptic glands of the stomach; and, at an autopsy, the mucous membrane of the stomach, beyond exhibiting extreme emptiness of its blood vessels, shows no appreciable pathological alteration. In other cases considerable œdema of the mucous membrane is discovered at the *post mortem* examination, which must have prevented the glands from furnishing a normal secretion. Finally, the contents of the stomach are sometimes found to be highly ammoniacal, showing that urea has made its way out of the blood into the secretions of the stomach, and has been transformed by the influence of the gastric mucus into ammonium carbonate, which, neutralising the acid reaction of the gastric juices, has deprived these of their

digestive virtue. The dyspepsia due to this last cause is generally brought about only a very short time before death.

The *vomiting* of renal disease, like the dyspepsia, owes its origin to a variety of causes which have been ably summarised by Bartels. In the first place, in cases of acute swelling of the kidney, or of violent inflammation of the pelvis, or of the ureter of one of these organs, the attendant vomiting may be looked upon as a strictly reflex act; it is excited by an irritant impression made upon the nerves—either those connected with the capsules of the kidneys, and therefore implicated in the tension to which these organs when swollen would be subjected, or those which run to the pelvis of the gland, and which may either be involved themselves in the process of inflammation, or have suffered directly from the same cause (a calculus for example) that produced this. In much the same way vomiting may succeed an inflammatory irritation of the peritoneal coat of the kidney, as in nephritic abscess, or in the course of the development of new growths, or after abnormal distension of the pelvis of the organ by pyelitis or hydronephrosis. In all these instances the vomiting is attended by pain of more or less acute kind, and usually paroxysmal in its nature—pain which subsides in one class of cases with subsidence of the swelling, and in the other with the abatement of the sharp irritation provoked by the presence of the calculus in the pelvis of the kidney. The vomiting of renal disease may be also due to œdema of the mucous membrane of the stomach. In such cases the vomiting, as a rule, takes place in the morning before breakfast, when a quantity of watery fluid of very low specific gravity (1,002) of a faintly acid reaction is rejected. The co-existence of anasarca, the absence of urinary constituents in the vomit, and the results of *post mortem* examination, justify the assumption that such cases are due to œdema of the gastric mucous membrane. It is likely that the fluids effused in the submucous membrane of the stomach transude into its cavity and by their mere volume excite the vomiting. Furthermore, in many cases of renal disease obstinate and irrepressible vomiting follows upon the retention of urinary materials in the blood, and is then symptomatic of uræmia. In many cases this troublesome and invincible symptom is the first warning of chronic uræmia, and the precursor of severe nervous manifestations, convulsions, and coma. This uræmic vomiting produces serious effects upon the general state of nutrition by interfering with the assimilation of food already ingested, and usually causing it to be rejected as soon as it is swallowed. It is frequently associated with the greatest possible dislike to all solid articles of food, and especially to meat, and hence the rapid deterioration of strength observed from the moment this urgent symptom appears. It is probable that this form of vomiting is to be referred to direct irritation from the impure state of the blood, of those nervous centres which co-ordinate the act of vomiting.

Diarrhœa may also arise as a complication of renal disease. It occurs more rarely than vomiting, but is quite as obstinate and exhausting. The loss of strength is due to the rapidity with which the intestines are emptied, whereby the absorption of the food taken is prevented. The diarrhœa in renal cases may arise from a variety of causes, quite apart from that process of ulceration in the intestines which occurs so very frequently as a complication of the amyloid degeneration of the kidney. The diarrhœa may be, in the first place, due to œdema of the mucous membrane of the intestines, and then occurs only in the dropsical forms of renal disease. In such cases the stools are abundant, very watery, and but slightly coloured, and in this way the dropsy is either greatly reduced or completely removed. The diarrhœa may also be produced by the passage of urinary constituents into the intestinal canal and their conversion into ammonium carbonate. In such cases, which may be contemporaneous with vomiting of ammoniacal matters, the strong ammoniacal odour of the fæces may be sometimes recognised. Such a diarrhœa is practically one of the final symptoms.

There is yet another disturbance of the general nutrition which is of the utmost gravity in cases of renal disease—a *hæmorrhagic diathesis*. Petechial spots appear on the skin, and besides these the predisposition to bleed is evinced by spontaneous epistaxis, and by hæmorrhages from other tracts of mucous membrane—from that of the stomach and of the intestines, from the mucous membrane of the mouth, and by hæmoptysis. The most common source of all is the nasal mucous membrane. It is singular that there seems to be little tendency to bleed exhibited by the genito-urinary apparatus of women. After death there is not found the slightest anatomical lesion to explain these often profuse bleedings which must take place *per diapedesin*. It is only when this hæmorrhage takes the form of epistaxis that it may occur repeatedly during a prolonged period, and ought not therefore to be accepted as an immediate harbinger of death; when derived from other sources, and accompanied by petechiæ, it invariably indicates the near approach of dissolution. In every example of well-established hæmorrhagic cachexia which Bartels has observed, the patients whom it has befallen have not survived two weeks from the commencement of the bleedings—which may occur simultaneously from different mucous tracts. These hæmorrhages generally occur in cases of genuine contracting kidney (granular atrophy), and very rarely in dropsical persons. As to the exact cause of these hæmorrhages there is a division of opinion—some connect them with the increase of tension in the arterial system from the hypertrophied left ventricle, so frequently associated with contracted kidney, acting on an excessively thinned blood; others regard them as a sign of ammoniæmia. It is true that epistaxis occurs in guinea-pigs when solution of ammonium carbonate has been injected into their veins; and bearing on this matter

is a case in Huxham's Essay on Fevers, in which "vast hæmorrhages from the intestines, nose, and gums," were observed in a young gentleman of fortune and family who had acquired a pernicious habit of eating smelling-salts (ammonium sesquicarbonate). However, a case most carefully observed by Bartels does not find its explanation on either of these hypotheses—for in this case the hæmorrhages continued up to the very last, whereas the heart's power had sunk to a low ebb several days before death, the pulse being soft and empty; and in the same case the blood drawn from a vein before death, and tested in the most exact manner by the method of Kühne and Strauch, showed that there was not a trace of ammonia present.

[*To be continued.*]

NOTE.—The text of the essays of Professor Carl Bartels, of Kiel, and Professor Wilhelm Ebstein, of Goettingen, in Volume XV. of the "Cyclopædia of the Practice of Medicine," edited by Dr. H. Von Ziemssen, has been followed as closely as possible, with additional notes from all the best authorities on the subject.

A. W. FOOT.

TRACHEOTOMY IN LARYNGEAL TUBERCULOSIS.

SERKOWSKI has performed tracheotomy twice for tuberculosis of the larynx. One of the patients died three years after from advanced pulmonary phthisis; the other is still alive, seven years after the operation, and is apparently in good health except that there is dulness at the right apex, and thickening of the vocal cords. She wore the cannula for two years. He believes in this case that the opening in the trachea was not only of temporary benefit, but prevented the further development of tuberculosis. The opening should be large enough to allow the free passage of purulent secretion from the lungs and the admission of air. The operation is absolutely required when the larynx is more affected with tubercle than the lungs. The passage of the current of air through the inflamed and often narrowed larynx is no doubt a frequent source of irritation. Tracheotomy obviates this and prevents vocalisation, thus enabling the larynx to be in a state of comparative rest, which is most important, especially if there be extensive ulceration.—*Allg. Med. Chir. Zeit.* and *St. Louis Med. Jour.*

S. W.

SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P.

VITAL STATISTICS

Of Eight Large Towns in Ireland, for Four Weeks ending Saturday, December 28, 1878.

Towns	Population in 1871	Births Registered	Deaths Registered	DEATHS FROM ZYMOTIC DISEASES							Annual Rate of Mortality per 1,000 Inhabitants
				Small-pox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhoea	
Dublin, -	314,666	663	936	55	8	13	2	11	29	7	38·7
Belfast, -	182,082	455†	536†	—	—	18	—	15	19	6	39·1†
Cork, -	91,965	145	227	—	—	—	—	11	4	3	32·0
Limerick, -	44,209	55	134	1	—	—	—	5	2	3	39·3
Derry, -	30,884	53	44	—	—	3	—	—	1	1	18·5
Waterford, -	30,626	61	60	—	—	—	—	—	1	1	25·5
Galway, -	19,692	39	56	21	—	—	—	1	1	2	37·0
Sligo, -	17,285	36	28	—	—	—	—	2	—	2	24·8

Remarks.

In all the larger towns the mortality was extremely high, in consequence of unusually inclement weather. In Galway it was very high, owing to the continued fatality of small-pox, which caused 37·5 per cent. of all the deaths registered. Except in Derry, Waterford, and Sligo, which show a comparatively moderate rate of mortality, the births registered fell far short of the deaths. In Dublin 273 more deaths than births were registered during the four weeks. The return for Castlereagh No. 1 District, Belfast, was not received in the week ending December 14. The death-rate was 28·3 per 1,000 of the population annually in London, 22·3 in Edinburgh, and 32·5 in Glasgow. Amongst the deaths registered in London are included 528 caused by the sinking of the SS. "Princess Alice" in the Thames on September 2. If these are deducted, the rate in London falls to 26·3 per 1,000. Within the municipal boundary of Dublin it was 41·8, and in the Dublin registration district at large, when the deaths of persons admitted into public institutions from localities outside the district are omitted, it was 37·7. The deaths from zymotics were 149 in Dublin, or 21 more than the average number in the corresponding period of the preceding ten years (127·6). Small-pox shows a very decided increase—the deaths were 55 against 29 in the previous

four weeks, and the number of patients under treatment in the hospitals at the close of the period was 122, compared with 86 and 47 at the close of the two preceding periods. Of the deaths from fever, 8 were ascribed to typhus, 17 to enteric, and 4 to continued fever of undetermined type. In Belfast fever and whooping-cough, and in Cork whooping-cough, were fatal. The mortality from diseases of the respiratory organs again largely increased in Dublin, where the mean temperature of the four weeks was 31.3° . The total deaths from this class of affections were 359, compared with 202 and 102 respectively in the two preceding periods. They included 280 from bronchitis and 48 from pneumonia. The averages in the corresponding period of the previous ten years were—respiratory diseases in general, 168.8; bronchitis, 134.4; and pneumonia, 18.9 deaths.

METEOROLOGY.

*Abstract of Observations made at Dublin, Lat. $53^{\circ} 20' N.$, Long. $6^{\circ} 15' W.$,
for the Month of December, 1878.*

Mean Height of Barometer,	-	-	-	29.753 inches.
Maximal Height of Barometer (on 4th at 9 p.m.),	-	-	-	30.420 „
Minimal Height of Barometer (on 30th at 2 a.m.),	-	-	-	28.991 „
Mean Dry-bulb Temperature,	-	-	-	32.5°
Mean Wet-bulb Temperature,	-	-	-	31.7°
Mean Dew-point Temperature,	-	-	-	30.0°
Mean Elastic Force (Tension) of Aqueous Vapour,	-	-	-	.167 inch.
Mean Humidity,	-	-	-	91.0 per cent.
Highest Temperature in Shade (on 30th),	-	-	-	52.5°
Lowest Temperature in Shade (on 24th),	-	-	-	14.0°
Lowest Temperature on Grass (Radiation) (on 17th),	-	-	-	$14.9^{\circ} ?$
Mean Amount of Cloud,	-	-	-	56.5 per cent.
Rainfall (on 19 days),	-	-	-	1.616 inches.
General Direction of Wind,	-	-	-	W. and N.W.

Remarks.

A most inclement month, forming a fitting sequel to the coldest November experienced for more than seventy years. The mean temperature was 32.5° , or 8.7° below the average mean temperature (41.2°) of December in the last twelve years. Had it not been for the warm weather of the 30th and 31st, the mean temperature would have been below freezing point (31.3° for the first twenty-eight days of the month). Snow lay on the ground in Dublin and the neighbourhood from the 8th to the 27th inclusive, and at one time the depth of snow even in the city was upwards of half a foot. Until the 25th the general distribution of atmospheric pressure caused gradients for N.W. winds over the United Kingdom, and bourrasques travelled to S.E. or E. across North Western Europe, as had been the case in November. In Dublin a slight fall of snow on the morning of the 8th, and heavy hail and snow storms on the

9th, 10th, and 11th, ushered in a severe frost, which was only interrupted by a slight thaw on the 13th, to be renewed with greater intensity, until it finally broke up on the 25th. On the 16th, even t 1 30 p.m., when the sun shone brightly, the thermometer stood at $25\cdot4^{\circ}$, while it sank to $15\cdot6^{\circ}$ the following night, a thick rime being deposited on trees, &c. During the night of the 17th a heavy shower of *rain* was succeeded by a very dangerous "glazed frost" (Glatteis). Further falls of snow on the 20th and 22nd were followed by intense frost on Christmas Eve—at 9 30 a.m. the sheltered thermometer stood at $14\cdot0^{\circ}$, or $18\cdot0^{\circ}$ below freezing point. The highest temperature during the day was $20\cdot6^{\circ}$. At night a thick smoke fog hung over Dublin, the frost continuing intense. On Christmas morning, however, a fresh S.E. wind—part of the circulation round an area of low atmospherical pressure which was approaching the British Isles from S.W.—sprang up and caused a rapid rise of temperature and a gradual thaw. S.W. gales and great warmth for the season prevailed on the 30th and 31st. On the 30th large ice floes floated down the River Liffey. A faint aurora borealis was seen at 9 30 p.m. of the 16th. Lunar halos appeared on the 6th and 11th; lunar coronæ on the evening of the 7th. Fogs were of frequent occurrence in the cold period from the 12th to the 24th. Hail fell on five days, and snow or sleet on twelve days. On the night of the 18th thunder and lightning occurred at many British and Irish stations.

RAINFALL IN 1878,

At 40, Fitzwilliam-square, West, Dublin.

Month				Total Depth	Greatest Fall in 24 Hours		Number of Days on which $\cdot 01$ or more fell
				Inches	Depth	Date	
January,	-	-	-	1·557	·318	3rd	20
February,	-	-	-	1·576	·826	12th	14
March,	-	-	-	1·157	·200	28th	17
April,	-	-	-	2·350	1·102	29th	16
May,	-	-	-	4·540	1·220	10th	23
June,	-	-	-	5·058	·980	3rd	19
July,	-	-	-	·650	·324	26th	9
August,	-	-	-	4·641	1·106	13th	22
September,	-	-	-	1·684	·479	29th	16
October,	-	-	-	2·095	·965	7th	16
November,	-	-	-	1·338	·556	9th	11
December,	-	-	-	1·616	·284	11th	19
Total,	-	-	-	28·262	—	—	202

PERISCOPE.

Edited by G. F. DUFFEY, M.D., F.K.Q.C.P.

PYROGALLIC ACID IN PSORIASIS.

DR. A. JARISCH (*Pharmaceutische Post*) reports his complete success in the treatment of psoriasis by pyrogallic acid. At first he used an ointment containing 20 per cent. of pyrogallic acid ; this was, however, found to produce excoriations. Hence he has reduced the ointment, as ordinarily used, to the strength of 10 per cent., and in some cases he uses one of 5 per cent. only. If spread on muslin, and then applied, it must be still further diluted, otherwise it acts as an irritant. Aqueous solutions should contain about 1 per cent. Pyrogallic acid acts not as rapidly as chrysophanic acid, but it is equally certain in its results.—*London Medical Record*, December 15, 1878.

A NEW SYMPTOM OF ADHERENT PERICARDIUM.

DR. L. RIESS, Physician to the Berlin General City Hospital, has a paper in the *Berliner klinische Wochenschrift* for 23rd December, 1878, with the above heading. Dr. Riess says, "It is acknowledged that the diagnosis of adhesions of the pericardium is uncertain in more than half the cases which come under our notice. All the symptoms regarded as characteristic of this condition are far from being constant, especially the systolic contraction of the thoracic wall so much relied upon ; and it often occurs at the *post mortem* examination that adhesions are found which were not suspected during life. It is, therefore, of the greatest importance to collect characteristic phenomena, even although they may only be present in exceptional cases, in order, if possible, through the multiplicity of the symptoms, to compensate for what is wanting in their constancy. I think I may regard as a rare and hitherto unnoticed sign of pericardial adhesion the occurrence of a metallic ring in the heart-tones, produced by resonance in the adjacent stomach." Dr. Riess then narrates three cases in point, and says:—"In all three cases we have the phenomenon of a metallic resonance proceeding from the stomach, and caused by the heart-tones; while simultaneously we have the adhesion of the pericardial layers with close attachment of the outer pericardium to the diaphragm—this condition having been ascertained by *post mortem* examination or by marked symptoms during life. To my mind it is the close proximity of the stomach to the heart, produced by the intimate attachment between pericardium and diaphragm, which is one of the chief factors in causing

the resonance. That, however, the phenomenon was not always present in these cases—in the first case it existed only shortly prior to death—is a circumstance which does not disprove the explanation I have given, and cannot cause any wonder when the complicated nature of the conditions necessary for the production of such a resonance is considered. . . . What diagnostic importance the symptom has depends upon the relative frequency of its occurrence; and the object, therefore, of this communication is to invite further observations as to the coincidence of metallic heart-tones with adherent pericardium.”

H. R. S.

THYMIC ACID AND SULPHO-THYMATE OF SODA.

A THERAPEUTICAL study of these substances has been published by Dr. V. Cozzolino, in the *Giornale delle Scienze Med.*, of Naples. Thymic acid has been advantageously employed locally in diphtheria, and is superior to carbolic acid for this purpose, as it is much more agreeable to the patient, and does not produce nausea. For ozæna Dr. Cozzolino considers it the best article he has tried; he uses an emulsion with gum arabic, one part of the acid to one thousand of emulsion, and injects the nostrils. Combined with unguentum petrolei, or dissolved in alcohol and glycerine, he applies it in favus, tinea, herpes, pityriasis, &c. In gastric catarrh from ferment, putrid diarrhoeas, intestinal mycosis, and the like, he gives it internally with excellent results. He also praises the sulpho-thymate of quinia as an efficient antiperiodic.—*Med. and Surg. Reporter*, Nov. 16, 1878.

ON SUPERNUMERARY BREASTS.

THE following are the conclusions which Dr. Godfrain arrives at, in a carefully worked-out thesis upon this subject:—1. Supernumerary breasts are rare. 2. They are of the same structure as the normal breast. 3. They are met with most frequently two in number, less often one, and rarely three. 4. Their most common seat is the axilla, and this site is explained by the frequent existence of axillary prolongations of the gland. 5. Some of the supernumerary breasts have, as points of departure, a pediculisation, and a migration of the glandular lobules—analogueous to the migratory tumours of the breast. 6. These supernumerary breasts may exist with or without nipples, and in the former case the milk secreted resembles that of the normal breast. 7. The above characters of the supernumerary breasts enable us to distinguish them from tumours; but in some cases, in which consecutive alterations take place, their diagnosis may become very difficult or even impossible.—*Med. and Surg. Reporter*, Nov. 30, 1878.

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PART I. ORIGINAL COMMUNICATIONS.

ART. X.—*Illustrations of the Use of Chrysophanic Acid in some Diseases of the Skin.** By WALTER G. SMITH, M.D., Univ. Dubl. ; F.K.Q.C.P.I. ; Assistant Physician to the Adelaide Hospital.

I PROPOSE in this paper to make a few remarks relative to the use of chrysophanic acid in some diseases of the skin, and hope to elicit the experience of those who have tried the new remedy. My observations will be restricted to the class of parasitic diseases, and I will not now touch on the important practical question of the application of this acid to the non-parasitic diseases further than to express my conviction that the evidence in favour of its utility in psoriasis appears to be unanswerable, and that Neumann's statement—that the therapeutics of skin diseases have, for the last ten years, been enriched by but few remedies which have been crowned by so eminent a success as chrysophanic acid—can scarcely be accused of exaggeration.

If we regard the cutaneous affections commonly recognised as due to a vegetable parasite, they may be arranged, from a therapeutic point of view, in an ascending scale of increasing difficulty of cure, thus:—

1. *Tinea circinata*, or ringworm of the non-hairy parts of the body, most cases of which are easily and permanently curable.

* Read before the Medical Society of the College of Physicians on Wednesday, February 5, 1879.

2. *Tinea* (pityriasis) versicolor, the chloasma of English authors.

3. *Tinea tonsurans*, or ringworm invading parts where hairs are well developed, often an obstinate and perplexing disease to conquer.

4. *Tinea favosa*, or favus, the most intractable and the most disgusting of the ecto-parasitical affections.

Although *tinea versicolor* is usually a trivial affection, and quickly curable by the ordinary remedies, yet, from carelessness and other reasons, it is sometimes difficult to eradicate completely; and we must all feel indebted to Mr. Balmano Squire for introducing and popularising chrysophanic acid as an efficient stimulant and parasiticide.^a

The following cases, in brief summary, illustrate its use in this form of disease:—

CASE I.—A medical friend, of a not very robust frame, consulted me about two years ago for an eruption of *tinea versicolor* extensively diffused over the chest and back. He improved slightly under treatment with corrosive sublimate, soft soap, &c., but the affection persistently relapsed, and, being anxious to get thoroughly rid of it, he wrote to me last summer inquiring about the use of chrysophanic acid. I strongly advised him to try it, and he accordingly ordered the ointment as prescribed by Mr. Squire. Shortly afterwards he again wrote to me to say that the ointment had “made a perfect cure” of his skin, after having applied it once only to the back, and three times to the chest. He adds—“I tried everything I could think of before—even linimentum iodi, and strong mercurial ointment—without the slightest benefit.” Wishing to ascertain if a permanent cure had been effected, I lately (December, 1878) made inquiry, and learned that the chrysophanic ointment completely cured him for the time; but, he writes, within “the last four or five weeks I see small spots, the size of peas, on my chest of the (*tinea*) versicolor, which I intend to attack some fine day with the remains of the ointment; now that I have the cure in my own hands, it makes me careless. Any amount of washing, or baths, or soap, has not the very remotest effect on the disease.”

CASE II.—In September, 1878, a former pupil brought his wife to me on account of an extensive outbreak of *tinea versicolor* upon the chest, back, and side of the neck, which had developed three years previously after an attack of measles. She was of a brunette complexion, and appeared in excellent general health, and was within a few weeks of her

^a *Essays on the Treatment of Skin Diseases. No. IV. On the Treatment of Psoriasis by an Ointment of Chrysophanic Acid. By Balmano Squire, M.B. London: J. & A. Churchill. 1878.*

first confinement. The following ointment was prescribed:—Chrysophanic acid, gr. 30; spirit of chloroform, ʒi.; simple ointment, ʒi. Mix. An immediate good effect was observable, and in the course of three weeks she wrote to express her thanks for “the rapid cure of the unsightly brown spots.” “I am glad,” she says, “to be able to tell you that they have entirely disappeared. The outer skin is now peeling off, leaving the under one quite fair and white.” In a recent letter (December 14th), three months later in date, she writes:—“I am indeed glad to be able to tell you that your cure has been a lasting one. I have never had the slightest return of the brown marks. My neck and chest are perfectly white and fair, and my friends and relatives can hardly believe their eyes when they see my chest *now* compared to what it *was*.”

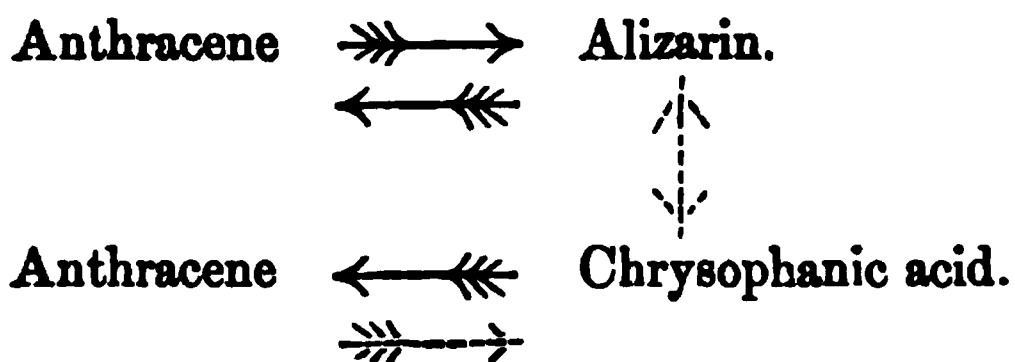
The third case of tinea versicolor, which I will presently adduce, possesses some interest from the results of a novel therapeutic experiment made in connexion with it.

In an interesting paper (*Edin. Med. Journ.*, July, 1878) Dr. James Adams relates two cases of psoriasis in which, in the pursuit of an ingenious hypothesis, he was led to employ *alizarin* as a substitute for chrysophanic acid. The results were equally effective, equally decided, if not so speedily apparent; and he anticipates “as possible and probable that alizarin may either prove a cheap substitute for chrysophanic acid, or that it may form a basis or a link in the artificial production of chrysophanic acid at a cheap cost.”

Alizarin is the colouring principle procured from madder, and, until a few years ago, was a costly product obtainable only from the natural glucoside in the madder plant. But, in 1869, Graebe and Liebermann added another to the many triumphs of modern organic chemistry, and, at the same time, revolutionised a great branch of commercial industry by their important discovery of the synthesis of alizarin from anthracene ($C_{14}H_{10}$), one of the hydrocarbons in that still unexhausted treasury—coal tar. The pigment is now made on a vast commercial scale. Alizarin belongs to the same group of bodies as chrysophanic acid—viz., the anthracene group, and, according to some, is isomeric with it. It possesses likewise acid properties, and presents some features of analogy with chrysophanic acid. Thus, for example, with potash either substance forms a compound of a fine purple or red colour.

The chemical synthesis of a natural organic pigment—the first synthesis of its kind—has reduced the price of alizarin to a very moderate cost, and it is probable, as Dr. Adams suggests, that ere long a process will be devised that will convert anthracene or alizarin

into chrysophanic acid, and so lower the cost of that remedy as to bring it within the reach of all. The gaps that yet remain to be bridged over by fresh discoveries are indicated by dotted arrows in the subjoined scheme of the compounds just referred to:—



I was struck by Dr. Adams' observations, which appeared to open up a fresh path in cutaneous therapeutics, and accordingly I determined to put the newly suggested remedy on its trial at the first suitable opportunity.*

CASE III.—A few weeks ago, S. B., a young man, aged nineteen, from the county Mayo, who had been under my care some months previously for right facial paralysis, again came up to town and sought advice for an extensive eruption of tinea versicolor, which appeared some twelve months ago on the shoulders, and had gradually spread downwards. The patient was of a fair complexion, with light hair; and the eruption, which was of a dusky brown colour, extended over the front of the body as low as the umbilicus, and a little lower behind. Along the arms it had crept a short distance below the bend of the elbow.

The eruption was almost absolutely symmetrical in every small detail on each side of the body, and it occurred to me that this was a favourable case for testing if alizarin were possessed of parasiticide properties. For as the use of chrysophanic acid in various chronic skin affections was an extension of its original application to the treatment of parasitic maladies, so it appeared reasonable to try if conversely alizarin, which had been recommended as a stimulant in psoriasis in lieu of chrysophanic acid, might not prove serviceable as a parasiticide. In this latter capacity alizarin has not yet been tested, so far as I know; and without claiming more for a single case than it is worth, I desire to record it in the hope of inviting others to institute further experiments.

Treatment was commenced on the 4th of December. A preliminary scrubbing with soft soap was ordered, and the following two ointments

* Dr. Janisch, clinical assistant to von Hebra, has also announced his intention to test alizarin clinically in skin disease on the strength of its chemical analogy to chrysophanic acid (*Wiener mediz. Blätter*, No. 7, 1878, quoted in *Pharm. Journ.*, July 27, 1878, and *Dubl. Med. Journ.*, Sept., 1878).

prescribed:—For the *left* side of the body, alizarin ointment, ʒi. of commercial artificial alizarin to ʒi. of lard. For the *right* side of the body, chrysophanic ointment, ʒi. of the acid to ʒi. of lard. The wide extent and symmetrical distribution of the eruption combined to render the case a fair one for comparison of the effects of the two drugs.

On December 9th, up to which time the ointments had been rubbed in regularly twice a day, the following points were noted:—*Right* (chrysophanic) *side*.—Skin brownish and reddened, especially about the neck; eruption considerably lessened over the front of the body. *Left* (alizarin) *side*.—Skin stained deep yellowish brown; eruption not materially altered in extent or character. Neither ointment causes any sensible irritation. The ointments were subsequently prepared with vaselin.

12th. *Right side*.—Many brown patches to be seen below the nipple, and about the clavicle and shoulders a number of vivid red patches. On the back the eruption seems to be as abundant as before treatment. *Left side*.—Skin stained orange brown, and a crop of minute dark red specks visible on the arm, the mammary region, and on the back. In the centre of each speck a hair stump appears. Four days later the eruption was much less apparent on the alizarin side, especially in front, and the skin presented no evidence of desquamation. On the chrysophanic side the skin was freely desquamating above and in front, and was tender when washed. The eruption stood vividly out.

Dr. Head saw the patient a few days afterwards, when the eruption had nearly faded away on the left (alizarin) side, and also to a less extent on the right (chrysophanic) side. The line of demarcation, back and front, between the two sides, dusky-red and tawny-yellow on either side respectively of the middle line, was sharply defined, showing that the ointments had been strictly applied according to directions. It was also noticed that the side of the shirt in contact with the alizarin ointment was very slightly stained, while the chrysophanic ointment had, as usual, stained the shirt deep brownish-red. On the 21st inst. I took scrapings from the skin of both sides, and examined two samples of each under the microscope. In neither of the alizarin specimens could I recognise the characteristic grouping of the microsporon fungus, and only some minute spheroidal bodies (of doubtful nature) were seen on one or two epidermal fragments. Neither could any trace of mycelium be detected in the chrysophanic scrapings, but the minute spheroidal bodies were much more numerous. He was obliged to leave Dublin for his home on December 23rd, and at that date the skin on the left (alizarin) side was smooth and natural looking, slightly yellow, and presenting a mere trace of the eruption below. The skin on the right (chrysophanic) side was dusky-red, and presented more evident remains of the eruption.

On the 13th January, 1879, the patient wrote to me in reply to some queries:—

“I am delighted to inform you that both ointments have taken wonderful effect, each having produced a perfect cure. Of the two the alizarin ointment is the better, in my estimation, for no one could apply them more justly than I have done; and to-day, after the sharpest inspection, I could not detect the least particle of the disease, whereas on the right side, to which the yellow ointment had been rubbed, there remain about thirty little spots, the greater part of which are on the stomach. The eruption caused by the yellow ointment disappeared gradually as the skin inside became healthy, but I have remarked that where the spots remain there had been no eruption or irritation whatsoever, and undoubtedly the ointment has been distributed equally. As regards the staining of the clothes, until they have been washed the alizarin ointment appears to stain them more, but washes out more freely than the yellow ointment, which latter cannot be removed thoroughly; but I have been told by the washerwoman that bleaching takes the best effect.”

These cases taken together go towards confirming Mr. B. Squire's observations upon the use of chrysophanic acid as a speedy cure for *tinea versicolor*, while any appearance of a relapse can be at once met and instantly stamped out by a timely recourse to the remedy.

Before concluding, I will add a few words in reference to the common forms of ringworm.

The treatment of *tinea circinata* of the body is a simple matter, and we have numerous remedies at our command by which a good result is, as a rule, easily and rapidly attainable; and chrysophanic acid gives, as has long been known by Indian practitioners who are familiar with Goa powder, satisfactory evidence of its parasiticide power. For example, Mr. C. N., a medical student, consulted me in July, 1878, for ringworm. Several rings of *tinea circinata* appeared about October, 1877, on the side of his neck. No treatment had been adopted, and in July, after a duration of nine months, the rings had partially coalesced and formed gyrate patches. Chrysophanic ointment (3i. to ʒi.) was prescribed, and was diligently applied by Mr. N. Within a week the disease had totally disappeared, and no recurrence of it has taken place.

But in the management of that plague of practice, *tinea tonsurans* or ringworm of the scalp, I am unable to say that chrysophanic acid fulfils the desideratum of an efficient and speedy cure, and in this respect my experience coincides with that of Dr. Radcliffe

Crocker, who treated twenty cases of tinea tonsurans with chrysophanic acid:—"The results were by no means so satisfactory; in most of them other remedies had been tried, and the disease had existed for some time. When tinea circinata and tinea tonsurans occurred together in the same case, the ringworm on the body was readily cured. Of the whole twenty, two were cured completely—one in six weeks, the other in two months. In seven there was certainly improvement—that is, where there were several patches some were cured, but in the other patches the hairs were still diseased. The remaining eleven cases were only slightly improved, though the treatment was kept up for at least three months and even longer" (*Lancet*, January 27, 1877).

The acid is, moreover, open to certain disadvantages now tolerably well known. Of these the staining of the skin and dyeing of the hair are the least, for these quickly disappear when the use of the drug is suspended, and by simple precautions the discoloration of the patient's pillow, linen, &c., can be avoided; but the irritant qualities of the acid are more marked, and in more than one case I have been compelled to discontinue its use, even when freely diluted (20 grs. or less to ℥i.), on account of the cedema, irritation, and pain provoked by it.

To sum up, I think that the knowledge we have at present warrants me in saying:—

1. Chrysophanic acid is a powerful local stimulant—not, however, tending towards vesication or ulceration. Its action in this capacity is best illustrated in psoriasis.

2. It also possesses undoubted parasiticide power.

3. It is a most efficient remedy in two parasitic affections—viz., ringworm of the body, and tinea versicolor.

4. In ringworm of the scalp it frequently fails owing to causes that militate against all remedies as yet tried.

5. In favus it has not, so far as I know, as yet had a trial.

6. As a destroyer of animal parasites its efficacy remains to be proved. It would probably be of service in some cases of scabies and in pedicularia.

7. Internally, as Dr. Ashburton Thompson has shown, it is an emetic purge.

8. Alizarin, upon several grounds, appears to deserve a more extended trial in diseases of the skin.

ART. XI.—*Two Cases of Hystero-Epilepsy.** By WILLIAM JOSIAH SMYLY, M.D., Univ. Dub.; F.R.C.S.I.; Assistant Physician to the Rotunda Hospitals.

FEW subjects which come within the domain of medicine have of late excited greater interest than that of hysteria-major, hystero-epilepsy, or epileptiform hysteria, as the affection is variously denominated. The number of cases published in our journals during the past twelve months, and the hot contests between those whose opinions differ upon the various complex questions which the subject suggests, sufficiently attest the truth of this assertion. It is not, I imagine, as has been stated, "a disease of extreme rarity," for the fact that so many cases have been assembled at one time in a single institution—namely, the Salpêtrière Hospital in Paris, that so many have been recorded in these countries during a single year, and the fact that two examples have fallen within my own personal experience, seem to contradict the assertion. In its symptoms the affection is most appalling to onlookers. Its course is very uncertain, and does not, according to Professor Charcot, tend towards recovery. In some instances, as in one of my own cases, and in some recorded in Professor Charcot's lectures, it even terminates fatally. The method of treatment proposed for its cure is one of the most extraordinary and novel in modern therapeutics, and claims more from our credulity than the notorious system of Hahnemann. Here we have only to discover by experiment the metal to which the patient is susceptible, and its internal administration is the most certain means of cure—not only so, but we have also the means of telling whether the cure is complete or only apparent—for, by placing a dish of the same metal upon the forehead of your patient, the symptoms will, in the latter case, immediately reappear.

Although I must confess to being sceptical on the subject of metallotherapy, and incline to the opinion of those who consider that the results obtained by some are owing rather to mental than to physical causes, still, as Dr. Sigerson has said in a recent paper on this subject, "it is right to remember that we have here not simply the assertion of *an* investigator" to deal with, "but the labours of a commission appointed by an eminent society to examine and test his statements."

* Read before the Medical Society of the King and Queen's College of Physicians in Ireland, on Wednesday, February 5, 1879.

The symptoms of the disease are well known through the admirable translations of Professor Charcot's lectures by our fellow-citizen, Dr. Sigerson, in which, and in his original papers, will be found, I believe, almost all that is at present known upon the subject.

I shall not, therefore, occupy time by further preamble, but proceed to the account of two cases which have come under my personal observation.

The notes of the first case were taken in the year 1876, before I had any knowledge of the works of Charcot or Burq; and although wanting in many symptoms, which might have been discovered had they been looked for, are still interesting, I think, as the observations of a mind unprejudiced by preconceived opinions.

CASE I.—Miss G., aged twenty-six, menstruation always irregular; but otherwise, until three years and a half ago, she enjoyed perfect health.

As the history of the case extends over so long a period, I shall content myself with the description of a single series of convulsions and a short *résumé* of the development and progress of the disease.

Having, for some time, wished to be present during an entire attack, and having had a free day on the 10th April, 1876, I called at the patient's lodgings at half-past eleven in the forenoon. I found her, having just finished breakfast, lying upon a sofa, perfectly conscious and capable of conversing, but interrupted frequently by what seemed to be laryngeal spasm, which was accompanied by a peculiar noise, something like a dog coughing. She also vomited occasionally, the quantity brought up each time being small, and the act unattended by nausea.

At noon a wet cloth was placed over her eyes, when the barking sound immediately ceased, but she began to clear her throat violently and to expectorate copiously. Moaning in a piteous manner, she soon lapsed into a state of apparent unconsciousness. The left fore-arm was rapidly pronated and supinated, and the corresponding foot synchronously flexed and extended.

How long this might have continued I cannot say, for it was suddenly cut short by her aunt tickling the side of her face. At this she shuddered, as if frightened, and started into a sitting posture, her knees drawn up under her chin, and her arms crossed under her thighs. She turned her face towards the back of the sofa; and a chair, with a pillow upon it, having been placed behind her, she began to kick against the back of the sofa, slowly and gently, with the regularity of a machine.

Having continued thus for upwards of an hour, she got lengthwise upon the sofa, down towards the lower end, her legs hanging over the arm of it. Suddenly she flung her heels over her head, assuming very much the

position of a Chinese juggler when balancing something upon his feet—standing upon the nape of her neck, the body and legs rigid and perpendicular to the horizon, and the arms crossed behind her back, so as to afford a broader basis of support. This position was maintained for fifty seconds, when she came down suddenly and heavily without flexing a joint. The legs were then thrown backwards and forwards over her head about ten times. She now assumed a different attitude, with flexed thighs and extended legs, the toes almost touching the sofa above her head, both upper and lower extremities being agitated with clonic spasms. These two positions—the first attended by tonic and the second by clonic spasms—with the violent to-and-fro movements interposed, were regularly alternated for half an hour, each being maintained, however, for a longer period each time, until at last she stood two minutes and a half upon the back of her neck; then, throwing her feet one hundred and ten times over her head, she lay exhausted and gasping for breath.

Having rested a minute, she began again, but the paroxysm assumed quite a different character. The legs, instead of being closely pressed against each other, during the to-and-fro movement, were twice crossed during the ascent, and twice during the descent, and her body being at the same time rotated from side to side, the feet were shot out right and left alternately. The intermissions were not attended, as in the first stage, by complete relaxation, but the upper extremities were spasmodically flexed, the fingers on the hand, the hand on the forearm, and the latter on the arm.

At three o'clock the paroxysm became most furious; the feet were thrown backwards, forwards, and from side to side, the legs alternately crossed, extended, flexed; the hands entangled in her clothes were wildly flapped on either side; her face hideously contorted, the under lip thrust out, and the saliva blown in foam from her mouth; respiration laboured and rapid, and an incessant barking sound proceeded from her throat.

After a few moments' rest she covered her face with her hands and wept piteously; then, raising herself on her elbows and crawling to the upper end of the sofa, laid her head against the side and passed into a state of syncope.

Soon, however, the convulsions returned, and she repeated the performance as described, and this she did a third and fourth time with unvarying precision. After the final paroxysm she rested for a longer time, but instead of beginning again she grasped her throat with her right hand and rapidly turned her head with a semi-rotatory motion, the hair standing out around it like the strands of a trundling mop; she then tossed it to and fro, knocking it against the sofa with horrible violence. She became quiet, opened her eyes, gazed fixedly at one corner of the room as if entranced, then, shooting out her hand to grasp at something, she became conscious.

She seemed surprised to see me there, and said she was tired and thirsty; cold water was brought, with which she rinsed her mouth, but dared not swallow any. It was now eleven o'clock at night, and the performance began at noon.

Calling next morning, I found that she had passed the night in a state of trance, so that she could not be removed into bed.

Calling again in the evening, I could hardly believe that she was the same person whom I had seen that morning or the day before. She surprised me by her cheerfulness, knowing, as I did, that she had not eaten for thirty-six hours, and that the greater part of the scanty meal which she had then taken had been vomited soon after. She complained of pain in the left ovarian region, which was also tender; in the epigastrium and left side of the face under the clavicle, and at the angle of the scapula of the same side. These symptoms had been constant for months. If the tender spot at the angle of the scapula should happen to be pressed, even by a wrinkle of the bed clothes, she would pass at once into a cataleptic condition, remaining so perhaps for hours. There was a spot also on the top of her head which must not be touched; it was about the size of a florin, and the hair growing upon it was never brushed or combed, for should it be touched she would at once begin the spasms as already described, which would continue for eleven hours. I once touched this spot, by accident, in the middle of a fit; she immediately became conscious, but began all over again, with extraordinary precision, so that the fits lasted fourteen hours that day. These phenomena were also excited by the act of deglutition.

Could she have subsisted without swallowing, she would have had no spasms. For a fortnight that she was supported by nutrient enemata alone, she had not a single attack.

Sometimes she complained of severe pain in the left side of her face, which soon would become hot, red, and swollen; and, after a period varying from a fortnight to six weeks, these symptoms would subside.

The assigned cause of this illness was shock to her nervous system by the sudden death of her father, to whom she was greatly attached. This occurred whilst she was standing by his bedside. She seemed unable to leave the spot where she was, and was at last carried to her bed, where she "swooned" away, and did not recover consciousness until the next day. Shortly after this, during a violent paroxysm of sobbing, her inspiration assumed a loud crowing sound. She soon acquired a habit of tapping the ground with her left foot, which subsequently became paralysed. She, however, partially recovered the use of the limb; though at the time I knew her, she described it, as well as the hand and arm on the left side, as "dead feeling," though occasionally she could use the hand in sewing.

Believing that her disease was chiefly laryngeal, she came to Dublin in

1873 to consult my brother, who found the larynx healthy. She was also seen by Dr. Kidd, who declared that, with the exception of tenderness in the ovarian region and some abrasion around the os uteri, the generative organs seemed to be sound.

From the first nervous shock the menses were not seen; her friends were, however, occasionally alarmed by the presence of blood in the urine, which was probably vicarious; other urinary symptoms, such as dysuria or suppression, were occasionally observed.

The therapeutic agents from which she derived most benefit were blisters. There was no doubt that she was better so long as a blister remained open. For some time chlorodyne, and afterwards croton chloral in large doses, had some effect. Chloroform stopped the contractions, but as soon as it was discontinued, they commenced again exactly where they had been interrupted. Bromides, foetid gum resins, and valerian, were useless—the cold bath, electricity, and the strait-waist-coat, positively injurious.

In December, 1877, whilst I was absent from home, violent diarrhoea set in, alternating with vomiting, both of which resisted treatment. She died in perfect consciousness, complaining of pressure on the chest and violent pain in the bowels. There was no blood or symptoms of dysentery.

The following points in the case I would direct attention to as resembling the symptoms in Professor Charcot's cases:—

1st. Ovarian hyperæsthesia, with numbness of the same side.

2nd. The convulsions divided into stages:—

a. The epileptiform, which was badly marked.

β. "Phase des grands mouvements," which could not have been more extravagant.

γ. "Phase des attitudes passionnelles," represented, I think, by the period when she raised herself upon her elbows, retreated towards the sofa end, and covering her face wept piteously.

3rd. The sensitive regions, or epileptogenic zones, one on the top of the head, and the other somewhere in the pharynx or œsophagus.

The assigned cause, a sudden fright; and the action of chloroform, controlling for the time, but not curtailing, the movements.

The second case is still under observation.

CASE II.—The patient, Mrs. T., aged twenty-two, is a married woman, and has had one child. She has been married about two years, and her relatives informed me that she is of an exceedingly nervous and passionate temperament. She has been hystero-epileptic for about a year; cannot assign any cause for the origin of her disease, but says that "the fits

come on when she is annoyed." She was treated for some nervous affection in the Mater Misericordiæ Hospital when a little girl.

She first came under my notice amongst the out-patients attending the Rotunda Hospitals' Dispensary about the middle of last November; was then suffering from an abscess in the left mammary gland; she also complained of "nervousness" and uncontrollable shaking of the right arm. Her eyes having been covered, a needle was thrust into the back of the right hand so that it bled. As she evinced no sign of pain, I asked did she feel anything touching her? She said "no." The left hand was then pricked, but she immediately withdrew it with an exclamation, and forcibly uncovered her eyes. I tried to persuade her to allow me to prick the right hand again whilst her eyes were open, but she would not permit it. I found, on further examination, that there was complete anæsthesia on that side, affecting the skin and mucous membranes, and almost complete analgesia. The special senses were also affected—taste and hearing being absent—and there was well-marked acromatopsia. When the left eye was blindfolded she described brown as black, and red as brown; a green book with gold letters she said was white with black letters. There was ovarian tenderness on the right side.

I have never been present during a convulsive attack, but they have been described to me by her relatives, and also by the midwife who attended her in her confinement—the latter, who is an intelligent woman, said that at first she thought that they were puerperal convulsions, but that she kicked about much more; after the fits she slept profoundly for a long time.

The patient herself informed me that she knew by a peculiar sensation rising from her stomach when a fit was impending, and she immediately lay down.

I was not able, therefore, to employ personally the method proposed for controlling these paroxysms, by pressure in the ovarian region, but I endeavoured to teach her sister how to employ it; and she told me that on two occasions that she had done so, the attack was immediately interrupted, but as soon as the pressure was removed the symptoms immediately reappeared.

I completely failed in all my attempts to cause a transference of the anæsthesia to the opposite side. I first tried the effect of a large electromagnet, which was kindly lent to me by Dr. Coppinger, of the Mater Misericordiæ Hospital, for the purpose. The patient being blindfolded, the poles of the instrument were approached to the skin of the forearm, but although we waited a considerable time and tried them at various distances, there was no result. A solenoid, formed of an insulated wire coiled in a spiral and connected with a single galvanic cell, was afterwards placed upon the index finger of the affected hand, but still the result was negative.

On the 20th December I first mesmerised my patient. This was effected by holding a bright object within about six inches of her eyes, and bidding her look at it intently. Soon her eyelids drooped, the head fell forwards upon her chest, and she appeared to be fast asleep. The limbs were quite relaxed and anæsthesia complete, a needle being driven through the web between the fingers of either hand without causing pain. I now blew suddenly into her face, hoping to arouse her, but instead of this she became rigid. I raised her arms and they remained outstretched—she was evidently in a cataleptic condition. I then placed the electrodes of a Brammer's battery in her hands; this I used at its maximum power, but it was some moments before it had the slightest effect. She suddenly became conscious, however, and looked round as if surprised and startled.

Having read Dr. Carpenter's paper in the *British Medical Journal*, in which he describes the wonderful effects produced by a portmanteau key upon an hysterical patient, I determined to repeat his experiments. Having placed the ring of a pair of dressing forceps upon the first finger of the anæsthetic hand, I informed the bystanders that sensation would soon return, and such was the case. I then changed it to the second finger, saying it will now depart again, and so it did. I have since, on several occasions, tried to repeat this experiment, but have not succeeded.

The symptoms, as above described, are not constant—on one or two occasions there was no anæsthesia; most of the other symptoms were, however, present, and the patient was easily mesmerised, after which hemi-anæsthesia remained for some time.

On the 24th of January she came to me with symptoms of paralysis of the facial nerve, and protrusion of the tongue, which she declared she could not keep in her mouth. I have searched in vain for sensitive regions, but have not found any.

INJECTION OF CARBOLIC ACID IN PRURIGO.

FLEISCHMANN vaunts the efficacy of subcutaneous injections of a 2 per cent. solution of carbolic acid in cases of prurigo. At first he injects subcutaneously half, and subsequently the entire of, the contents of an ordinary Pravaz's syringe. The injections are made in those places where the itching is greatest, and the number of the injections should be regulated by the gravity of the case, and especially by the intensity of the itching. In Fleischmann's cases the total number of the injections varied from three to fifteen. One injection at least was made every second or third day. The relief, followed later by a perfect cure, was never delayed longer.—*Rev. des Sci. Méd.*, Jan., 1879.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Les Tumeurs Adénoïdes du Pharynx-Nasal; leur Influence sur l'Audition, la Respiration, et la Phonation, leur Traitement.

On Adenoid Tumours in the Naso-pharyngeal Space; their Influence on Hearing, Respiration, and Phonation, and their Treatment.
By Dr. LOEWENBERG. 8vo. Pp. 75. Paris: Adrien Delahaye & Co. 1879.

IN this *brochure* Dr. Loewenberg gives to medical science a very valuable contribution on an obscure but most important subject. If we suppose the soft palate raised to a horizontal position, the region which lies above it is termed the naso-pharyngeal space. Although affections of the mucous membrane lining this space are extremely common, and give rise to symptoms distressing both to the patient and his friends, and liable to cause serious secondary diseases in other parts, still there can be no doubt but that they have been almost universally overlooked by the profession. Indeed, it is probable that few others than aural surgeons direct their attention to the matter at all, and this is much to be regretted, for aural complications appear in only a portion of the cases, while there are none of them which do not properly come within the scope of the family physician. The disease of which our author treats is one of the most formidable which occurs in this region.

Adenoid tumours of the naso-pharyngeal space were first observed by Czermak,^a and then by Türck and Semeleder. In a memoir published in 1865,^b Dr. Loewenberg cited five cases of the affection; and an excellent article upon the subject by Dr. Meyer, of Copenhagen, appeared in 1873.^c

Dr. Loewenberg subjected the tumours which he extirpated to microscopical examination. They were covered with a stratified

^a Du Laryngoscope, &c. Paris. 1860.

^b Archiv für Otologie, Vol. II., p. 103.

^c Ueber Adenoide Wucherungen in der Nasennachenhöhle. Archiv f. Otologie, Vol. I. (N. F.), p. 241.

epithelium, of which the superficial layers were cylindrical, and for the most part ciliated. The mass of the tumours was composed of adenoid tissue, of which the trabeculæ became visible only after the lymphatic cellules which filled its meshes had been removed with a camel's-hair pencil. The drawing in the excellent handbook of Cornil and Ranvier gives a good idea of the structure of these tumours.

In the normal state adenoid tissue, richly supplied with follicles, is freely distributed over the mucous membrane of the naso-pharyngeal space, especially towards the entrance of the Eustachian tube, and in Rosenmüller's fossa, and also on the roof of the pharynx, and at the part contiguous to the posterior wall. It forms at this place, from the orifice of one Eustachian tube to that of the other, a layer of varying thickness, which may be regarded as a special organ, and which has been named the pharyngeal tonsil.* This mass adheres intimately to the fibro-cartilage which unites the pharynx with the base of the cranium.

The parts to which we have just referred, being by preference the seat of adenoid tumours, it becomes more than probable that they are in fact a simple hypertrophy—a morbid development of the tissue which enters so largely into the structure of the pharynx. Indeed, in some instances, it is difficult to decide whether we have before us an extreme, but yet physiological, development of the pharyngeal tonsil, or a commencing hypertrophy of it. From a clinical point of view, therefore, a diseased condition need not be admitted, unless the increase in volume of the tonsil be such as to interfere prejudicially with the functions of the naso-pharyngeal cavities.

Those tumours, which are situated in front of the Eustachian tube, consist of adenoid tissue, with a variable proportion of connective tissue. In the normal state, however, adenoid tissue does not exist at this point; hence, in the diseased state, it is present as a neoplasm, not merely as a hypertrophy.

These tumours may be divided into—(1) sessile or flat tumours, and (2) pedunculated tumours. It is often difficult, in an early stage of the affection, to determine to which group the tumour before us belongs, and even in advanced stages intermediate forms must be recognised. Occasionally, when the pharyngeal tonsil is extremely hypertrophied, it presents the appearance of a group of long vegetations, which, growing close to each other, and in a parallel

* Luschka, in Max Schultze's *Archiv f. microscop. Anatomie*. 1868.

direction, may seem, at first glance, to be but a single sessile tumour. These adenoid tumours may attain such dimensions as to completely fill the naso-pharyngeal space. For the most part they are multiple. Sometimes one is surprised by finding considerable difference in size between the tumours at consecutive examinations. This can only be accounted for by supposing a greater or less efflux of blood in such vascular pathological productions.

Dr. Loewenberg was the first to establish the close anatomico-pathological affinity between these adenoid tumours of the naso-pharynx with the granulations so well known on the posterior wall of the throat. He observed several cases in which these two morbid productions coexisted, and has even seen instances of intermediate degrees between these two productions exist in the same individual.

With regard to the etiology of these growths, some have thought that their development was very specially encouraged by the severity of northern climates; others, again, that they were to be found chiefly among the lower grades of society. Dr. Loewenberg, however, is unable to adopt either of these theories, and refers their development principally to two causes—namely, lymphatic temperament and hereditary tendency. He cites some interesting cases which have come under his care to establish these points. He mentions that adenoid tumours are very frequently found in persons affected with cleft palates.

When these adenoid tumours have obtained a considerable development they interfere with respiration, pronunciation, and hearing. The permanently opened mouth, the peculiar mode of speaking, and the deafness, combine in so characteristic a way as almost to make the very appearance and speech of the patient diagnostic of the affection.

When, however, the disease has not yet reached its summit, or when, from their position, the tumours do not encroach upon the nasal passage or the Eustachian tube, they do not give rise to such apparent symptoms. There are, then, other symptoms which, if less characteristic, are still most valuable, while of course, the special physical examination is the only method which can reveal the true nature of the affection.

Symptoms caused by Adenoid Tumours in their Incipient Stage.—A pharyngo-nasal catarrh accompanied by abundant secretion is present from the commencement of the affection. On examining the back of the pharynx a mass of greenish mucus is seen covering

it, and these masses are frequently tinged with blood, and are expectorated in large quantities by mouth and nose. Granulations of the buccal pharynx are often present. Sometimes there is difficulty of respiration through the nose even in this stage, but not commonly. These symptoms do not by themselves afford any certainty in the diagnosis, which can only be arrived at by direct examination; but yet it is important to be aware of the signs of these early stages lest, as we see every day, they should be referred to some general cause. Being once put on our guard, the digital examination and the rhinoscope will afford us certain intelligence of the state of affairs.

Symptoms caused by Adenoid Tumours in their Advanced Stages.—These are many and vary much according to the position of the growth. The latter may interfere with respiration and phonation if they tend to occlude the naso-pharyngeal passage; or, by pressing on the Eustachian tube, they may compromise the functions of the ear. If the growths quite fill the naso-pharyngeal space, all the symptoms will be present in their most extreme degree. It may be well to take each group of symptoms in rotation according to their cause.

Suppression of Nasal Respiration and its Consequences.—In those cases where the tumours attain a size sufficient to obliterate the posterior openings of both nasal fossæ, the air is obliged to pass during the act of respiration by the mouth, which therefore remains always open. Even an incomplete obstruction of these passages is sufficient to make them inadequate to perform their physiological function. This substitution of the mouth for the purpose of breathing entails a series of ill effects:—

(a.) The change in the expression of the face consequent upon the open mouth, already referred to.

(b.) The effects upon the pharynx. In breathing normally by the nose the inspired air undergoes two important changes while passing through the narrow winding passages of the nostrils—it is warmed and moistened. On the other hand, when breathing is carried on by the mouth the inspired air impinges directly upon the posterior wall of the buccal pharynx, cooling and drying the parts which form the entrance to the larynx and trachea. It is especially the mucous membrane of the buccal pharynx which becomes affected, not only in its epithelium, but even as deep as the superficial layers of the true membrane itself. The chronic irritation which is produced and kept up by buccal respiration has been recognised by many

authors. Dr. Loewenberg, however, thinks that some have gone too far in imputing the origin of granular pharyngitis to this mode of respiration—a point upon which he has the support of Dr. J. Patterson Cassells' (Glasgow) opinion. He thinks a certain confusion in the matter has arisen as regards cause and effect—granular pharyngitis, he believes, provoking buccal respiration, and not the latter giving rise to the former. Moreover, it is acknowledged that persons come daily under observation who respire by the nose, and who yet are affected with granular pharyngitis. Still Dr. Loewenberg does not deny but that the drying and cooling of the mucous membrane of the buccal pharynx do, to a certain extent, aggravate a pre-existing granular pharyngitis by interfering with the circulation of blood in the superficial layers of the mucous membrane. In this way a vicious circle may be set up.

(c.) The effects upon the nasal fossæ. The pituitary membrane undergoes considerable degeneration, occasionally of a colloid nature. In a large number of cases there is a chronic rhinitis with increase of the secretion. The mucous membrane becomes swollen, and that which covers the inferior turbinated bones so thickened that the posterior extremities of those bones protrude into the nasal pharynx, making special operations necessary. This tumefaction in its turn interferes with the expulsion of large masses of mucus which are secreted.

(d.) Influence on the senses of smell and taste. The sense of smell is generally weakened, and in advanced cases often even quite destroyed. This depends upon the degeneration of the Schneiderian mucous membrane in which the olfactory nerve terminates, as well as upon the mechanical obstruction to the passage of a current of air sufficient to carry volatile substances to those terminal filaments. The relations between the senses of smell and taste are intimate. They depend, Dr. Loewenberg thinks, upon the current of air which passes out through the nostrils after having passed through the pharynx, where it is impregnated with the effluvia from the aliments which have been swallowed. If a person with normal senses of smell and taste close his nose, immediately the taste becomes blunted, and the only powers of taste retained are those simple ones of sugar, salt, &c.

(e.) Influence upon the purity of the inspired air. During its passage through the nostrils the air is to a great extent deprived of many of its impurities, such as dust, smoke, &c., which become

entangled in the mucous lining of the tortuous passages. Pasteur^a and Tyndall^b have shown to what a degree the air we breathe is impregnated with organic and inorganic particles; and, indeed, we need only examine the nasal mucus after an evening spent at the theatre, or in a café, or other crowded resort, to show of what use our nostrils are in this matter. Our author here refers to the admirable treatise of Dr. Cassells, entitled "Shut Your Mouth and Save Your Life,"^c and which we can heartily recommend to the perusal of anyone interested in the subject.

(f.) Influence upon the development of very young children. Valuable as nasal respiration is for health at all ages it is of capital importance in infancy, particularly during the first year of existence, as we know by the important researches of Rayer^d and Billiard.^e In the commencement of life, infants, as Kussmaul^f has shown, do not know how to breathe by the mouth, even if the nose be closed; and when they sleep with the mouth open they still breathe by the nose. When at last the infant has learned to breathe by the mouth, it becomes liable to all the ill results consequent upon the habit which have been enumerated, and also to others peculiar to this early age—thus Paul Niemeyer attributes to buccal respiration certain attacks of false croup which are noticed during the early parts of the night.

(g.) Influence in arresting the development of the thorax and in producing deformity of it. In many of the younger persons who came under our author's care for this affection he found the chest flattened in a degree quite out of proportion to the general development of the individual, while in some there was a marked depression in the region of the eighth, ninth, and tenth ribs (*côtes moyennes*). Dr. Loewenberg grants that we cannot seek the origin of these conditions in the habit of buccal respiration, for the mouth provides a way amply capacious for the abundant supply of air to the lungs. He thinks it is in the incipient stages of the affection, before buccal respiration is completely established, that these thoracic complications arise. The volume of air admitted by the nostrils is then not sufficient to fully inflate the lungs, and the expansion of the thorax is incomplete. Inasmuch, however, as this

^a *Annales de Chimie et de Physique*. 3^{re} Série. T. LXIV. 1812.

^b *On Dust and Smoke*.

^c *Edinburgh*. 1877.

^d *Note sur le Coryza des Enfants à la Mamelle*. 1820.

^e *Traité des Maladies des Enfants*. 1828.

^f *Zeitschr. f. rat. Medicin*. 1865. P. 225.

takes place during the period of growth, permanent ill results are the consequence. Dupuytren observed considerable deformity of the thorax in persons affected with great hypertrophy of the tonsils. Dr. Loewenberg thinks it probable that in the cases that great surgeon refers to there were adenoid growths in the naso-pharyngeal space in addition to the enlarged tonsils he mentions.

(h.) Influence on the pronunciation. Certain letters, called resonating, require for their pronunciation that the nasal passages be quite free, in order that sonorous vibrations may be produced in them. Hence the impossibility on the part of our patients to pronounce the letters *m* or *n* when the affection has advanced far enough to obliterate the nasal passages. If they try to pronounce these letters they substitute others for them—instead of *m* they say *b* and *n* becomes *d*—e.g., *same* is pronounced *sabe*, and *nose* is transformed into *doze*. If to these substitutions be added a peculiar tone, or rather want of tone, a deadening or veiling of the voice consequent upon the absence of resonance in the nose and nasal pharynx, the characteristics of nasal speech are completed. Dr. Loewenberg then proceeds to analyse the physiological conditions of phonation, and to determine why, in the cases in point, certain letters are substituted for others. Space does not permit us to follow him in this interesting inquiry.

Influence on the Larynx and Singing Voice.—We touch here, Dr. Loewenberg says, upon an obscure subject. It has frequently been observed that paralytic aphonia, which resisted the electric treatment usually so efficacious in those cases, was not relieved until finally either an hypertrophied tonsil or some adenoid tumours had been removed. When this complication has been relieved the aphonia disappeared spontaneously, or under the influence of the induction current which previously had proved impotent. This can only be explained by a reflex action of the pharyngeal affection upon the innervation of the muscles of the larynx. Gerhardt goes so far as to say that every irritation of the pharynx is capable of producing aphonia by a reflex path. Sometimes the presence of the tumours, in place of suppressing the voice, merely diminish its compass—preventing, when large, the production of high notes. Prof. Meyer, of Copenhagen, observed a case in which the voice was raised a tone and a-half immediately after removal of the growths. This effect of the vegetations upon the voice may be due to the obstacle they offer to the elevation of the soft palate. However, it must not be forgotten that an obstacle

of such size is not necessary to produce the same restraint, for we know that the pressure of a simple granular pharyngitis is often sufficient to prevent emission of the high notes. 'The question, our author observes, is not so simple as it at first seems ; and he thinks it is necessary in every case to combine the inspection of the pharynx and the laryngoscopic examination with a rhinoscopic examination.

Influence of the Adenoid Growths on the Middle Ear.—Three-fourths of these cases are accompanied, our author says, by inflammation of the middle ear. He has most frequently found chronic catarrhal inflammation of the Eustachian tube and middle ear. The suppurative form, with perforation of the membrane, is very much more rare. These tumours act injuriously upon the ear by keeping up in the pharynx an irritation which is readily propagated to the middle ear, or by mechanically obstructing the entrance of the Eustachian tubes. In the latter case they interfere with the renewal of the air contained in the middle ear and in the mastoid cells. A diminution in the volume of that air then takes place by the process described by Dr. Loewenberg^a as "quasi-respiratory interchange." For the theory universally received until then Dr. Loewenberg substituted a new one supported upon a physical and physiological basis, and which has been made still more probable by the results he has obtained from two new methods of treatment deduced from the theory itself. The occlusion of the Eustachian tube by these growths impedes, moreover, the draining off of liquid secretions in cases of inflammation of the middle ear. The detention of these secretions thus induces perforation of the membrana tympani, and even more serious disorders.

Dr. Loewenberg regards the prognosis of affections of the ear in such cases as very favourable—more so, indeed, than similar affections dependent upon other causes, for the starting point of the affection can be successfully attacked, a condition which is frequently wanting in other instances.

Diagnosis of Adenoid Tumours of the Pharynx.—The accurate and definite diagnosis rests alone on direct examination, of which there are two methods—the rhinoscopic and digital.

Although some authors regard rhinoscopy as always extremely

^a De l'Échange des gaz dans la caisse du Tympan ; considérations physiologiques et applications thérapeutiques. Par le Dr. Loewenberg. Progrès Médical, le 24 février. 1877.

difficult, and even impossible in some cases, our author affirms that it succeeds often at the first attempt, that it is difficult to perform in other cases, but that he has never found it absolutely impracticable in any of the numerous cases where he has required its aid. The two great obstacles to be overcome are the tongue and the soft palate. Many patients find a difficulty in flattening the tongue so that it will not diminish the space already so confined; and this troublesome peculiarity seems more common amongst females than in males. If in such cases one tries to depress the tongue with a spatula it often becomes violently curved, making all examination impossible. Dr. Loewenberg explains to such persons, by help of a looking-glass, what it is he requires; and when they have sufficiently practised the necessary evolution he proceeds with his examination.

The second obstacle is more serious. In many persons the least contact of the mirror with the base of the tongue, or still more with the soft palate, gives rise to violent contractions, which make it necessary to desist, lest vomiting be caused. In other persons the moment they open the mouth they raise the soft palate, thus completely precluding the rhinoscopic examination. In order to avoid the latter inconvenience Czermak advised that the patient should pronounce a nasal vowel, which would render the elevation of the soft palate impossible. Dr. Loewenberg finds that this expedient often fails, and prefers the following plan:—He directs his patient to breathe by the nose, which it is impossible for him to do unless the communication between the buccal pharynx and the nasal pharynx is open—in short, unless the soft palate is lowered. It is often very useful to make the patient use the naso-pharyngeal douche previous to rhinoscopic examination, in order to clear away all mucus masses.

The digital method of examination is the second which Dr. Loewenberg employs for ascertaining the presence of adenoid growths. Directing the patient to breathe by the nose, he passes his index finger round the soft palate, and with rapidity and gentleness examines the posterior openings of the nasal fossæ, the lateral and superior surfaces of the pharynx, the entrances of the Eustachian tubes, and the posterior surface of the soft palate. It is important that the nail of the exploring finger be pared closely, to avoid as much as possible wounding the parts. It has been proposed that in this digital examination, as also in rhinoscopy, the patient's head should be inclined forwards, in order that the soft

palate might fall forwards. Dr. Loewenberg does not support this suggestion, because when the head is inclined forwards the vertebral column is brought closer to the soft palate, and thus the antero-posterior dimension of the nasal pharynx is diminished, and the difficulty of the examination consequently increased. Our author prefers to make his patients incline the thorax forwards and the head backwards, thus combining the greatest distance between the soft palate and the back of the throat with pushing back of the cervical vertebræ. In this way we obtain the greatest amount of space which the anatomical relations will permit of. The digital exploration allows us to judge of the condition of the postero-superior wall of the pharynx and of its lateral regions—regions not easily seen with the rhinoscope. The finger also informs us concerning the mutual relations of the parts, and whether the vegetations be hard or soft. This method of examination is also of great use because it succeeds at the first attempt, while the results of the rhinoscopic examination have often to be waited for. The rhinoscope, however, shows us the condition of the mucous membrane, its colour, and its secretion, and it enables us to penetrate deeply into the nasal fossæ. It also informs us of the existence of small soft vegetations more surely than palpation does.

Dr. Loewenberg enters at considerable length into the differential diagnosis which these tumours involve, but we must ask our readers to be content with a simple enumeration of the conditions which may be confounded with them. They are:—Chronic simple coryza, hypertrophy of the tonsils, nasal polypus, and polypi of the nasopharyngeal space.

Prognosis.—What becomes of these growths when they have attained their maximum size? A decided answer to this question can hardly be given, for the affection has not been long enough known to admit of any one case having been long under observation. The fact, however, that the disease is very much more common in youth than in middle or old age makes it probable that the tumours undergo atrophy after a certain time, and thus disappear, leaving after them, unfortunately, those secondary affections of more or less serious nature to which they gave rise.

In Dr. Loewenberg's experience, when the adenoid tumours have once been radically removed, they do not recur. The secondary affections of the ear are not difficult of treatment once the growths have been removed. If the tumours are removed at an early stage it may be possible to prevent the buccal respiration becoming

habitual, but after a certain duration of the malady restoration of the normal condition of the parts does not imply immediate restoration of their physiological functions. The soft palate frequently continues raised, probably in consequence of spastic muscular contraction, and nasal respiration continues to be impossible.

Treatment.—The treatment may be divided into:—*a.* Treatment of the general health ; *b.* Local surgical treatment; *c.* Treatment of consecutive affections.

Treatment of the general health.—Inasmuch as a large proportion of the individuals affected in this way are of a strumous diathesis, treatment directed to it will be found necessary.

Local treatment.—Cauterisation and ablation are the two methods employed by Dr. Loewenberg.

Repeated cauterisations are sufficient to destroy the tumours in some of the cases. The method requires, of course, infinitely longer time for a cure than instrumental removal; but when the tumours are sessile, or fixed by a wide base, offering but little hold for an instrument, cauterisation is the better plan. Sometimes also cauterisation must be adopted when the patient, or his parents, will not permit an “operation” to be performed.

The instrument which Dr. Loewenberg employs is a silver *porte-caustique* of cylindrical shape, 23 centimetres long and 4 millimetres in diameter. The part which is introduced through the mouth, as far as the nasal pharynx, in order that the whole of that region may be reached, has a curvature corresponding to the longitudinal axis of this cavity—so that when it is required to reach the summit of the pharynx, the operator need not come in contact with the other walls. This part forms, with the body of the instrument, an angle corresponding to that which the axis of the mouth and of the pharynx form with each other. At its extremity there is a protuberance in the form of a quadrangular prism 17 millimetres long and 4 millimetres broad, and provided on all its surfaces with small roughnesses which facilitate the adherence of the caustic. The handle of the instrument is curved downwards, so as to leave the field of operation free. Solid nitrate of silver is the only substance which Dr. Loewenberg has yet employed, except in the case of very young children, or very restless patients, when he uses the mitigated nitrate of silver (1 part nitrate of silver, 2 parts nitrate of potash). In actually making the application it should be done by aid of the rhinoscope, or guided by the index finger of the other hand—the soft palate being lowered in the way already

recommended. To avoid cauterising healthy as well as diseased parts, Dr. Loewenberg covers the tip of the instrument with a bit of gutta-percha tubing. Upon reaching the part to be acted on a slight pressure causes the tubing to start backwards, exposing the caustic. In withdrawing the instrument the tubing recovers its former position by rubbing against the wall of the pharynx. The tubing is changed for each patient.

In order, after the cauterisation, to prevent any excess of nitrate of silver from finding its way into the stomach or larynx, Dr. Loewenberg makes his patients apply a solution of common salt to the parts by the nasal douche, and by gargling; and he thinks it most important that the patients should be carefully taught the use of the douche before any other treatment is commenced. If instrumental removal of the tumours be adopted, the douche is of great use in arresting hæmorrhage, by the injection of astringents, and for clearing the field of operation by washing away mucus. The cauterisation is repeated as soon as the eschara has come away, which usually occurs in the course of twenty-four hours.

Instrumental removal of the growths has been performed in several ways. Sharp spoons, of shape suitable for the purpose, have been employed with great success. The tumours, when pedunculated, may be severed with an instrument similar to Wilde's polypus snare, or Maisonneuve's ecraseur. When sessile they may be crushed with a polypus forceps like that of Ch. Fauvel. Meyer, of Copenhagen, has devised a circular knife for the removal of such growths. The instrument is passed through the nostril corresponding to the position of the growth to be operated on—the latter being fixed by the index finger of the left hand passed through the mouth, and thus the ablation is performed. Dr. Loewenberg does not use this instrument, because it gives great pain in its passage through the nostril, and because it is difficult, in many cases, to reach the tumours with it. Our author prefers, of all these methods, the use of the sharp spoon. But he thinks it may sometimes be dangerous in wounding healthy parts when the patient is restless, or in penetrating deeper than the actual limits of the tumours. He therefore now employs an instrument of his own invention—a forceps with cutting branches—which he recommends as a most efficacious instrument in these cases, while with it the traumatism can readily be limited to the part actually operated on.

The galvano-cautery might be employed in these cases, but our author has not as yet tried it.

Dr. Loewenberg, at the conclusion of his *brochure*, devotes some pages to the treatment of those secondary affections already referred to, and of which the removal of the adenoid tumours is not always sufficient to the cure. Want of space unfortunately forbids us entering into a consideration of these important matters.

H. R. SWANZY.

Materia Medica and Pharmacy. By W. HANDSEL GRIFFITHS, Ph.D. Edited, and in part written, by GEORGE F. DUFFEY, M.D., Dubl. Dublin: Fannin & Co. London: Baillière, Tindall, & Cox. 1879. Pp. 309.

THE late much-regretted Dr. Griffiths held, in common with many other teachers, the view that the study of *Materia Medica* and *Pharmacy* should enter into the earlier portion of the curriculum of the student, and should be used as a stepping stone to the study of *Therapeutics* and *Practice of Medicine* in the later part of his course. In the present volume the subject of *Therapeutics* is designedly kept in the background, and the author has endeavoured to supply a book which should contain the chemical and pharmacal knowledge requisite for medical and pharmaceutical students.

We do not know in what state the manuscript was left by the author, who was taken away so unexpectedly, but there is abundant evidence to show that the editor's work has been no sinecure, and the publishers may be congratulated upon having secured the services of one so well fitted for the task, and upon the manner in which he has discharged it. The text is clearly printed, is very free from clerical errors, and even in the numerous chemical formulæ extremely few slips occur. The most useful feature of the book appears to us to be the tabular arrangements so freely furnished under the various drugs and Natural Orders, showing at a glance the preparations of the British Pharmacopœia, their strength, the doses, and, briefly, their action and uses. The work is divided into two parts—the first deals with *Materia Medica*, and the second with *Pharmacy*, *i.e.*, the Pharmacopœial preparations. This latter portion is mainly a reprint of a former publication, entitled "*Notes on the Pharmacopœial Preparations*," which met with a very favourable reception.

This subdivision seems to involve some inconvenience in the treatment of the chemical articles in Parts I. and II., respectively. For example, at p. 6 we have a table of the acids, excluding all the

dilute acids except two, and on p. 210 the rest of the dilute acids are given. Would it not have been simpler and more consistent to have placed them all together? Similarly, the inorganic "Liquores" are dissociated from their metallic radicals, described in Part I.; and, again, in Part I., the "characters and tests" of the chemical substances are separated from the description of their mode of preparation. All this necessitates cross references, and a double or treble entry in the index for many compounds. We venture to suggest that the next edition would be improved by recasting the plan so as to place all the inorganic articles alphabetically, completing the history of each article under one heading.

We can commend this volume to students and practitioners as one which conveys a large amount of reliable information in a clear and attractive form, and which will prove a useful companion to the Pharmacopœia.

A Practical Treatise on the Diseases of the Testis and of the Spermatic Cord and Scrotum. By T. B. CURLING, F.R.S.; Consulting Surgeon to the London Hospital, &c. Fourth Edition, revised and enlarged. London: J. & A. Churchill. 1878.

MR. CURLING'S work has in its previous editions dealt so fully and so well with the subject of Diseases of the Testis and its appendages that all the profession know and appreciate its merits. But little in the way of criticism, of praise or of dispraise, is left for us in noticing the fourth edition. The best compliment, perhaps, that we can offer is to direct attention to the fact that, while almost every organ in the body has attracted authors in number—some by the score—the testis has for upwards of thirty years remained the property of Mr. Curling; no writer has displaced him—we do not even know that any have made the attempt. In this edition the author adds descriptions of congenital hydrocele of the spermatic cord and of lymph scrotum—the first a rare curiosity, and the latter chiefly of interest to surgeons practising in India and to the pathologist. The most important changes in this edition are the rewritten descriptions of the structural disease of the testis. In these the pathological details are brought into accord with the histological advances of the present day, and many new and important facts are added. In effecting these changes the author has showed admirable judgment in preventing an overgrowth of the volume.

On the Surgery of the Face. By FRANCIS MASON, F.R.C.S.; Surgeon, and Lecturer on Anatomy at St. Thomas' Hospital; Hon. Fellow of King's College, London, &c. London: J. & A. Churchill. 1878.

MR. MASON'S lectures on the surgery of the face may have been very interesting to his hearers, but they make a very indifferent book. Indeed that anything else should be the case might fairly be wondered at when we find the following plan adopted by the author:—

"In bringing this subject before you I must beg your kind indulgence, inasmuch as I shall have necessarily to refer to many topics with which I feel sure you are all more or less familiar; and I wish to say, at starting, that my object is not to excite sensation, or to provoke controversy, by placing before you novelties, but it is rather to group together a number of cases I have culled from various sources, including many that have been under my own observation, and which have special reference to the surgery of a region which, from its conspicuousness, forms a very important part of the human body."

Such a plan, followed by a pleasant lecturer, might, with abundant and well-selected illustrations, render the course of lectures both interesting and instructive. Print the lectures, and omit the major part of the illustrations—all, in fact, of the living and of the pathological illustrations—and the book becomes disappointing in the extreme. The author indeed furnishes us with a good many "before- and after-operation pictures," and representations of many old and familiar operations which have been before the profession for many years; but, wherever we look for information on a special point, we are met with a reference to a specimen on the shelves of St. Thomas' Museum, a patient who was "shown to the Fellows," or, perhaps, to somebody's description in a journal—every other information is left to our imagination.

We are at a loss to know what has guided the author in his observations on skin diseases affecting the face. He deals with erythema, roseola, and urticaria, lichen, herpes, eczema, psoriasis, lepra, and parasitic disease, acne, boils, and carbuncles, and ulcers; but the student could not recognise any of these diseases from their descriptions, nor does the practitioner, we are bound to say, learn anything of value with regard to their treatment. Of lupus there is not a word either of description or treatment. The author is not fortunate in his chapters on facial tumours, or on facial injuries.

Foremost amongst the carcinomata he places epithelioma; and, while of the dislocations of the lower jaw he indicates but one variety, he puts forward as dislocation of the superior maxillary bones unmistakable instances of fractures of the bones of the face.

Clinical Lectures on Diseases of Bone. By C. MACNAMARA, Fell. Cal. Univ.; Surgeon to the Westminster Hospital; Surgeon-Major, H. M. E. Indian Medical Service; also of the Royal Westminster Ophthalmic Hospital. London: Macmillan & Co. 1878.

THE observation of the author, in opening the subject of the union of fractures, might well be placed on the first page of the book instead of nearer its end, for it would spare the reader a great deal of trouble in his reading—giving him a clear hint as to what parts of the book he might skip without fear of loss.

“The process by which fractured bones are reunited has been the subject of numerous treatises, opinions differing remarkably as to the reparative action, because authorities hold adverse views regarding the formation and growth of the osseous tissue, and it is consequently impossible for pathologists to describe changes with precision respecting which they are themselves doubtful.”

The author seems bent on taking an advanced position as a bone pathologist, but, as it were, oppressed with the subject, he determines, by the passage before us, to throw the weight of his imperfections off upon the physiologists and anatomists who have failed to supply him with a sound basis for his work. He, in the passage quoted, pretty clearly states we do not know how bone grows, and then, relieved of any difficulty, away he starts in the next sentence. “It is evident that the repair of a fracture is effected by a process identical with that of the formation of bone.” And so he builds his theory. Everywhere this fault spoils these lectures. We find, for instance, assertions such as that the cells of lacunæ of bone “are simply cartilage-cells encased in earthy matter;” “that the vitality of the articular cartilages mainly depends upon the supply of blood contained in the medulla, as that of the cornea does upon the surrounding vessels;” while in studying the disease known as chronic rheumatic arthritis, an opposite view being more convenient, the author does not hesitate to say:—“The subcartilaginous osseous lamina, depending as it does for its growth and

maintenance on the cartilage-cells, also degenerates." It is hopeless to attempt to unravel the mystery of the author's theories when they are so based. Many of the practical clinical illustrations are good and instructive, but we cannot endorse the idea that exercise of the bone whose medulla is diseased is harmless, nay, even useful, as long as the disease sticks to the medulla—this structure being distinct from the bone the latter will not harm, according to the author. Nor, again, will we admit that to induce ankylosis of a joint the absorption of its cartilage and sub-cartilaginous layer of bone is necessary; this would be to set aside the form of ankylosis long ago demonstrated by Cloquet as resulting from disuse. The author's lectures are of interest where he draws his illustrations from bedside experience in India, for they give us many important facts on the peculiarities of bone disease among people of whose bones, at least, we here know but little. Even in this branch the author shows but a limited knowledge of the distribution of disease, although his positive statements as to India may, we presume, be taken as correct. Writing of rickets he says:—

"Rickets is, in a great measure, an affection attending the advance of civilisation and the crowding of families into large cities. Thus, nearly 30 per cent. of the children admitted into the metropolitan hospitals are affected more or less with rickets. The same observation applies to the population of Manchester and other large towns, not only in England, but also in America and the Continent of Europe. The disease, however, is not engendered by poverty alone, because the children of the upper classes suffer from rickets in as high a ratio as their poorer fellow-countrymen. On the other hand, if we turn from Europe and America to the population of India—I might almost say of Asia—we seldom (and so far as my experience goes, never) meet with cases of rickets among the natives, however poor and degraded the race may be—in truth, the more debased the tribe, the nearer the lower animals in their habits, the less subject to rickets are they."

Is the statement true that English children of the upper classes suffer from rickets in as high a ratio as their poorer fellow-countrymen? Contrary to the statement of the author we have good authority for the assertion that rickets is a rare disease in America. Our American cousins will not thank the author for the compliment contained in the statement that this immunity is due to the near approach to the lower animals in their habits of life. Markoe, writing from mature knowledge of his subject, obtained in New

York, states:—"Rickets is a disease so rarely seen in our country that I have no experience which would entitle me to speak of it authoritatively from my own observation." And again, Gross says:—"In this country it has always been extremely uncommon, even among the lower classes, whose children are its most frequent subjects." In the face of these statements we need not delay to examine the author's theories further.

A Manual of Anthropometry. By CHARLES ROBERTS, F.R.C.S.
Pp. 118. London: J. & A. Churchill.

THIS work is intended to be a guide to the physical examination and measurement of the human body, and, as such, it is a portion of an introduction to a larger work in the course of preparation on the physical development and proportions of the body. Mr. Roberts has collected masses of statistics showing the average height and weight at different ages, and he has drawn up elaborate scales for measurement. The chapter on the relation of weight to height may be useful in estimating the physique of factory children. The mean height of a male 10 years old is 53 inches; the mean weight, 67 lbs. For each inch in height the weight is increased 4 lbs. from 10 to 15 years; 6 lbs. from 15 to 16; 8 lbs. from 16 to 19; and from 19 to 30 years of age there is 6 lbs. added to the weight, while the height remains stationary. We fear, however, that few will think the details of sufficient importance to carry out the complicated system of measurements which the author has compiled for the use of anthropologists.

Medical and Surgical Report of the Tyrone County Infirmary for the Year 1878. By EDWARD C. THOMPSON, M.B.; Surgeon to the Infirmary. Omagh. 1879. Pp. 11.

THE publication of this practical Report is very opportune at a time when such legislative measures are being talked of as would seem to threaten the very existence of the Irish County Infirmaries. Dr. Thompson does not deny that some change in the present law affecting the Infirmaries is required. He observes:—

"The legislation necessary, however, is a Bill to consolidate and place upon a firmer footing institutions which, I unhesitatingly assert, are more useful, and do far more solid good, than any others in the country. The

contemplated Bill, therefore, should be one founded on principles of justice and common sense. A large surplus remains from the disendowment of the Irish Church. To what better purpose, might I ask, could some of this money be devoted than the endowment of the county hospitals throughout Ireland? The first clause, therefore, of an Infirmary Bill should be provision for a fair endowment. This would relieve county taxation, and be very acceptable to the cesspayers. The second clause should bring the Infirmaries under the Local Government Board, or of a board composed—as in the case of the asylums—of two medical inspectors, which would be even a better arrangement. As county institutions they would, of course, be independent of the Board of Guardians. Other clauses should provide for a proper audit of accounts, for the constitution of a governing body, which might, with advantage, remain as at present, for the proper remuneration and superannuation of the surgeon and other officials, and for the repeal of the old Infirmary Bill, with its outlandish and century-old provisions. Legislation founded on these principles would infuse new life into the Irish Infirmaries, and make them more useful to the public and to suffering humanity.”

It is very truly pointed out by Dr. Thompson that every year the great amount of good effected by the Irish Infirmaries is becoming more apparent, and it is now generally conceded not only that it is a necessity to have in each county at least one well-provided hospital for the treatment of severe cases of injury and disease, but also, as a matter of common fairness, that the respectable farmers, traders, shop assistants, servants, &c., should have some place of shelter when overtaken by severe illness, and not be compelled to frequent the workhouse hospitals—institutions no doubt admirable in many respects, but very generally repugnant to the feelings of the better class of our people.

Dr. Thompson appeals with confidence—and well he may do so—to the present Report as an evidence of the extent of good effected by a County Infirmary, and the amount of human suffering and misery it is able to mitigate.

During the year 1878, 575 patients have been admitted into the Tyrone Infirmary; 37 remained in hospital from the previous year; 62 trusses were issued, and 192 patients, whose complaints were not of a very serious nature, were treated externally, instead of being admitted to the hospital, as would have been the case in previous years. In all, therefore, 866 patients were under treatment during the past 12 months. 99 patients, whose cases were considered either incurable, or more suited for a workhouse than

an Infirmary, were refused admission. 69 operations were performed, including many of a dangerous and important nature. Thus, amongst the capital operations there were 3 amputations of the thigh; 2 of the patients recovered. 26 fractures and 4 dislocations were admitted, and all made good recoveries. The cases under treatment were classed as follows:—Medical, 227; surgical, 292; ophthalmic, 41; skin affections, 35; and diseases of the nose, throat, larynx, &c., 17—in all, 612.

Only 9 deaths occurred during the year, or a percentage of 1·5 of the admissions. The average number of patients in the hospital = 36·5, the total number of beds being 50, and the cost per bed occupied, £41 6s. 3½d.; the cost per patient amounted to £2 9s. 3½d., and the average stay of each patient in the hospital 23½ days. Of the patients admitted 370 were men, and 174 women; 42 were male children, and 26 were female children.

We have gladly noticed Dr. Thompson's Report, and only hope that the surgeons to other County Infirmaries will follow his example by placing before the public a record of the noble work which is carried on day after day in these institutions.

A Manual of Operative Surgery. By LEWIS A. STIMSON, B.A., M.D., Surgeon to the Presbyterian Hospital, New York. Pp. 477. London: H. K. Lewis, Gower-street.

AMONG the handbooks of operative surgery this must take a first place. The surgeon will always find here aid sufficient to direct him in any operation when he is beyond professional help. The illustrations number nearly 500, and the details are usually ample. The chapters upon amputations, excisions, plastic operations, and operations upon the eye, are specially good. The author gives a very good view of European Continental Surgery, much of which will be new to those who have not the opportunity of consulting the works of the French and German writers. The most recent improvements are included. The weak part of the book is to be found in the first twenty pages, wherein Dr. Stimson tries to give an account of anæsthesia, arrest of hæmorrhage, treatment of wounds, sutures, and bandaging, and succeeds very indifferently. The subjects are too big to be disposed of satisfactorily in such a space; and they ought either to be excluded altogether or be properly dealt with. For the rest the book is, with some faults, an admirable record of operative procedures, and one which we very confidently recommend.

Tinnitus Aurium; or Singing in the Ears. Remarks on its Causes and Treatment. By W. DOUGLAS HEMMING, M.R.C.S.; Senior Assistant Surgeon, Central London Throat and Ear Hospital, &c. London: Baillière, Tindall, & Co. 1878. Pp. 22.

"THE cry is still, 'They come.'" Pamphlets like this, we presume, fulfil some mighty and useful function. The supply appears to be enormous, and consequently, we presume, the demand is of equal proportion. As a rule, it can hardly be from the profession that the demand comes, but we find that the laity are wonderfully conversant with this class of literature, and it is so extensively advertised in the leading newspapers of the day that we conclude the demand comes from them.

A characteristic feature of this class of publication is a preface, which principally consists of an apology for the appearance of the work, but the prevalence and heartrending character of the complaint which forms the subject of it have proved too much for the benevolent author; and although it has already appeared in one or, possibly, more journals, he feels compelled to reprint it. The author's name and address in full are almost invariably appended.

Though Mr. Hemming assures us he does not pretend to give much "novel information" in his paper, which is reprinted from the *Student's Journal and Hospital Gazette*, yet there are one or two points which strike us as coming under the head of novelties. We read at p. 7 that "deficiency of wax will cause dry rustling sounds in the ear, particularly during mastication, and sometimes singing noises (Harvey). Examination will show the meatus red, with patches of dried wax upon it, the membrana tympani being generally opaque. This condition results sometimes from bathing." This is one of the most antiquated theories to be found in the literature of aural surgery, but we confess we thought it was now generally regarded as utterly groundless; and to judge from the latest and most reliable works on otology, there is no proof that a deficiency of the normal secretion is ever followed by any injurious consequences. Even if it were the case, however, we are completely at a loss to conceive on what principle of therapeutics "weak astringent solutions, such as nitrate of silver or acetate of lead," instilled into the ears night and morning, are recommended (p. 13) as the treatment to be adopted. Another statement, which we venture to think is perfectly novel, is that "the hairs in the meatus may become elongated, and, passing across, interlace with cerumen, or

epithelial scales. In these cases the sounds will frequently resemble those of an *Æolian* harp." And then again, at p. 14:—"Where the tinnitus is of an *Æolian* harp-like character, and appears to depend on the abnormal growth of hairs in the meatus, these should be removed." Imagine any patient being such a Goth as to seek the aid of an aural surgeon to remove such a beautiful portable *Æolian* harp arrangement as this!

Excepting the remarkable passages we have just quoted, and a reference to the use of hydrobromic acid, the paper really contains nothing that is not to be found in any of the standard text-books on aural surgery.

Illustrations of Clinical Surgery. By JONATHAN HUTCHINSON.
Fasciculus xiii. London: J. & A. Churchill.

THE thirteenth fasciculus of this most valuable and interesting work has a short but useful chapter upon congenital coccygeal tumours. The cases on record are very few, and this contribution to the literature of the subject is of special importance. Mistakes have been made by confounding these tumours with spina bifida; but while the latter usually occurs at the upper part of the sacrum, the former spring from the parts between the coccyx and the rectum, and displace and surround these structures. In the author's case the infant died at three months. The tumour was the size of a child's head. The growth was found to be adherent to the bones, and the coccyx and part of the sacrum were embedded between the lobes of the tumour. One lobe projected into the pelvis, and was covered with peritoneum, and six inches of the rectum, much displaced downwards, were embedded in the tumour. The mass was composed of cysts of various sizes, from a pea to a chestnut. These cysts had smooth epithelial lining, and contained a glairy transparent fluid. Hutchinson believes that these coccygeal tumours have their origin in the structures first described by Luschka, and called the coccygeal gland. They are vascular plexuses closely allied to cavernous tissue. The discoverer suggested the same explanation, and Heschl has adopted a like view.

There are excellent illustrations of brain bruising from *contre-coup*, and very full notes. Mr. Hutchinson's example of tireless industry in note-taking and careful observation might well be followed by hundreds of the profession. If he had done nothing more than this to help the development of surgery, he would still deserve the thanks of his brethren.

PART III.
HALF-YEARLY REPORTS.

REPORT ON
MATERIA MEDICA AND THERAPEUTICS.*

By WALTER G. SMITH, M.D., Dublin; F.K.Q.C.P.I.; Assistant
Physician to the Adelaide Hospital.

- ART. 4. Calomel and Sugar.
„ 5. Incompatibles.
„ 2. Sodium ethylate.
„ 3. Thymol.
„ 1. Vaseline.

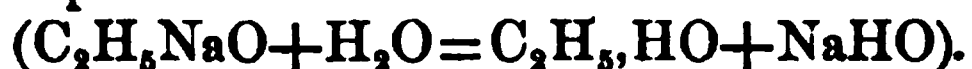
1. *Vaseline*.—This substance, which has justly gained a widespread popularity with English practitioners since its introduction from America some two or three years since, has not failed to attract attention in France, and we glean from the *Répert. de Pharm.*, Mai, 1878, a few additional particulars to those given in the Report for September, 1876. When crude petroleum is distilled, after the separation of the light oils, there remains in the apparatus a semi-liquid tar, which constitutes crude vaselin. In this state it has a disagreeable odour which discloses its origin; it is black, and has a strong taste. The tarry compound is heated in the open air, and then decolorised by animal charcoal, the product being commercial vaselin, which is a mixture of several hydrocarbons. When pure, vaselin is white, inodorous, and insipid. Its consistence is that of jelly, or of a very unctuous fat. It melts at 35° C., boils at 150°, distils at 200°, and burns without residue. A circumstance worth noting is that under the prolonged action of light it acquires a slight odour of petroleum. Insoluble in water, and sparingly soluble in alcohol, it dissolves freely in

* The author of this Report, desirous that no contribution to the subjects of *Materia Medica* and *Therapeutics* should remain unnoticed, will be glad to receive any publications which treat of them. If sent to the correspondents of the Journal they will be forwarded.

warm ether. It is very soluble in fats, volatile oils, chloroform, and sulphide of carbon. On its own part it sensibly dissolves iodine, bromine, sulphur, phosphorus, carbolic and benzoic acids, and almost all the alkaloids—especially strychnia and atropia. Its composition indicates that it is capable neither of becoming rancid nor of being saponified. Hence it may be utilised as an excipient for caustic alkalies, oxides, and metallic salts, and even for acids, in the cold, without being attacked by them, and without modifying their therapeutic properties. In various complex and easily alterable ointments the introduction of a certain quantity of vaselin will insure preservation and hinder rancidity. Dr. Galezowski, as the result of numerous experiments, affirms that vaselin is unquestionably superior to any other excipient hitherto used in the preparation of ointments—such as butter, lard, glycerin, and glyceroles.

2. *Sodium Ethylate, or Caustic Alcohol.*—At the meeting of the British Association for the advancement of Science, held at Liverpool in 1870, Dr. B. W. Richardson brought the ethylates of sodium and potassium, with some other of the ethylate series, into notice for practical use. In the same year he removed, by the use of sodium ethylate, a large nævus from a child who was under the care of Mr. Gay. The nævus had previously been subjected to treatment by the ligature and by other methods, including the free use of nitric acid, without success. In the course of the next year Dr. Brunton employed the ethylate for nævus with equal success.

Attention has recently been again called to these interesting compounds, and Dr. Richardson has accordingly contributed to the *Pharm. Journ.*, Dec. 14, 1878, a few practical remarks upon the use of the sodium alcohol (C_2H_5NaO), which we reproduce. From Dr. Richardson's researches it appears that the ethylates of potassium and sodium are bodies which, on being brought into contact with the moist living tissues, are decomposed—caustic alkali being produced and ethylic alcohol being reproduced by the extraction and decomposition of the water of the tissues.



Therefore four results ought to follow the application of an ethylate to a vascular living tissue—viz., (a) a removal or absorption of water from the tissue into the ethylate; (b) the destructive action of a caustic from the caustic soda that would be formed; (c) coagulation from the alcohol that would be reproduced; (d)

prevention of decomposition of the dead organic substance that would be formed.

Experience has shown that these results are attained; but if the ethylate be too concentrated the caustic action is over severe, and hæmorrhage may follow like as from an incised wound. This is specially the fact with ethylate of potassium, and hence Dr. Richardson prefers to recommend the ethylate of sodium of a strength to be presently named.

In order that the intention of the ethylates may be properly fulfilled, they must be sent out by the pharmacist as absolute alcohols, and for that reason absolute ethylic alcohol must be used in their manufacture. The sodium ethylate is best dispensed in a bottle furnished with a glass stopper, ending in a pointed glass rod which descends into the fluid. The liquid may also be applied very neatly by means of a clean quill, cut like a pen, and newly nibbed each time when used. The glass brush is very inadvisable. The small fibres break off, and, in one instance, a portion of fibre left on a surface touched with the ethylate caused great pain and trouble.

The formula for the ethylate is as follows:—Put half a fluid ounce of absolute alcohol (sp. gr. 0·795) into a two-ounce test tube. Set the test tube up in a bath of water at 50° F., and add, in small pieces at a time, cuttings of pure metallic sodium. A gas (hydrogen) will at once escape. Add the sodium until the gas ceases to escape, then raise the temperature of the water in the bath to 100° F., and add a little more sodium. When the gas again ceases to escape stop adding sodium; or if the fluid, which by this time will be of a gelatinous consistency, should crystallise, then stop. Afterwards cool down to 50° F., and add half a fluid ounce more of absolute alcohol.

3. *Thymol*.—Dr. C. Symes has made a few experiments upon thymol which are of interest, as this substance continues to be largely used. They supplement Mr. Gerrard's careful paper on the pharmacy of thymol, epitomised in the Report for July, 1878. It occurred to Dr. Symes to try if thymol and chloral hydrate, when rubbed together in a mortar, would produce a liquid similar to the well-known chloral-camphor. Experiment proved, however, that such is not the case; but if an equal quantity of camphor be added to the mixture, the whole at once liquefies and produces a powerful antiseptic. Two drops of this thymol mixture added to

a fluid ounce of urine containing pus, which was already in an incipient state of decomposition, at once, and for a considerable time, arrested putrefaction. Further experiments showed that thymol and camphor when rubbed together, in the absence of chloral hydrate, also become liquid, and that the proportions could be varied from 2 parts thymol and 1 of camphor to 1 part of the former and 10 of the latter, the result being a colourless syrupy liquid; equal parts of each gave very satisfactory results. The solubility of thymol in water is not greatly increased by this combination, but it is a very convenient form from which to prepare the ointment. Thymol-camphor can be mixed with vaselin, unguentum petrolei, or ozokerin, in almost any proportion; and an ointment prepared with 20 per cent. of the mixture, equal to 10 per cent. thymol, has been kept for some weeks without any separation whatever. A saturated solution of thymol in water (1 in 1,000) is found to be sufficiently strong for the spray during surgical operations, but for the throat and various other purposes it is often required stronger, and in such cases Dr. Symes knows of no better aqueous solvent than milk, which takes it up readily in almost any proportion up to nearly 10 per cent. of its weight; but it will be rarely required of such strength. Solution of borax is not a good solvent, but glacial acetic acid dissolves it most readily—a large proportion, however, separates on dilution. The acidum aceticum of the Pharmacopœia dissolves 2 grains in the fluid ounce. There appears to be some difference in the sp. gr. of thymol, arising probably from the source from whence derived; that described by Mr. Gerrard had a sp. gr. 1·028, hence was heavier than water, whilst the specimens Dr. Symes has met with have only a sp. gr. of 0·980 to 0·990, and float on or near the surface.—(*Pharm. Journ.*, Jan. 18, 1879.) The powerful antiseptic action of thymol, exceeding under some conditions that of carbolic acid, its small activity as a poison—about one-tenth of that of carbolic acid—and the absence of irritating effect when it is applied to the skin, all point to its use as a substitute for carbolic acid in the now well-known antiseptic treatment of surgical cases elaborated by Professor Lister. This substitution has been made with great success by Professor Volkmann, of Halle. For the spray solution, this surgeon uses a mixture of 1 part thymol, 10 alcohol, 20 glycerin, 1,000 water; but a solution in water only, which will not deposit, may be made by adding 1 part of thymol to 1,000 of hot water. For the gauze dressings used by Professor Lister, others were substituted, made

by saturating 1,000 parts of bleached gauze with a mixture of 500 parts spermaceti, 50 resin, and 16 of thymol. This prepared gauze is extremely soft and pliant, and sucks up blood and the secretions of the wound like a sponge. The fibres of the gauze being impregnated with spermaceti, cannot, of course, become saturated with the secretions, so that they do not become stiff. The present cost of thymol is about 5 times that of the best carbolic acid, but as one part of the former seems to do as much work as 25 parts of the latter, the advantage of price is on the side of thymol.—(*Chem. and Druggist*, March 15, 1878.)

4. *Calomel and Sugar.*—In two places attention has been drawn to the danger of keeping calomel mixed with sugar. Dr. Polk has observed a case of poisoning, presenting all the characters of corrosive sublimate poisoning, after the use of a powder composed of calomel and sugar, prepared a month previously, and in which chemical analysis revealed a notable quantity of sublimate. At Turin an analogous case of poisoning has been observed after the use of calomel pastilles. In each of these cases the question naturally arises—Did the calomel originally contain corrosive sublimate, or was there an after-formation of sublimate?—(*Rep. de Pharm.*, Oct., 1878, from *Pharm. Zeitsch. f. Russl.*) Instigated by the reports of such cases, Herr Slop was induced to make some experiments as to the conditions under which the mercurous is converted into the mercuric chloride, and its probable behaviour towards the juices of the stomach. The general conclusion arrived at was that mercuric chloride was formed in the presence of hydrochloric acid, organic acids, chlorides of the alkalies, and carbonates of the alkaline earths, the last being specially important, considering the frequency with which calomel is dispensed with magnesia. Turning his attention to sugar, Herr Slop examined several specimens of calomel pastilles, having sugar for their basis, that had been made more than a month. Some of these contained a considerable quantity of mercuric chloride, whilst others did not contain a trace. This difference he attributes to the latter having been prepared from a neutral refined sugar, while for the former probably a first crystallisation of beet sugar, still contaminated with the calcium hydrate introduced in the neutralisation and clearing of the juice, or a raw colonial sugar, which, as a rule, would be acid, had been used.

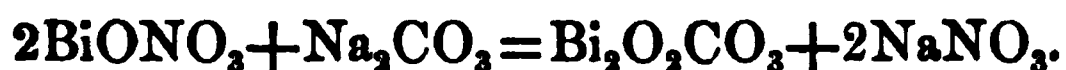
Mr. Langbeck doubts whether sugar perfectly dried and mixed

with well-washed calomel, and kept in a stoppered, coloured bottle, would change into glucose, and thus act as a reducing agent on the calomel, and some experiments which he has made confirm him in this opinion.—(*Pharm. Journ.*, August 31, 1878.)

5. *Incompatibilities*—(a) *Borax, Glycerin, and Bicarbonate of Soda*.—Hager points out that when a mixture of equal parts of borax and bicarbonate of soda is treated with water, no reaction is produced; but if glycerin be added, slight effervescence is caused, and carbonic anhydride is disengaged. By the aid of heat the liberation of gas is quickened, and analysis proves that exactly half the CO_2 of the bicarbonate of soda is eliminated; consequently, the solution acquires an acrid taste, owing to the formation of neutral carbonate of soda.—(*Rep. de Pharm.*, Sept., 1878, from *Pharmac. Centralhalle*.)

Messrs. Senier and Lowe have conducted some interesting experiments which lead at least to a very probable explanation of this reaction. To a solution of borax in water a few drops of tincture of litmus were added, colouring the solution deep blue. The addition of some glycerin to this solution caused it to change from blue to red, the characteristic wine red of free boracic acid. An experiment was then tried, using sodium monoborate in place of ordinary borax or biborate; in this case no red colour was developed. The ordinary acid borates of silver, diadmercury, lead, barium, and calcium were next employed. In each of these cases the red colour or acidity was developed in the same manner as when borax was employed. Water seems to act in a manner opposed to glycerin, for when added in excess to the acid solution the blue colour returns. It is said that a large excess of water renders even an aqueous solution of borax more alkaline to litmus. It is well known also that solution of sodium carbonate is decomposed by boiling with aqueous solution of borax. In this case carbon dioxide is evolved, and a more basic borate is produced. In like manner sodium bicarbonate or carbonate, or even calcium carbonate, is decomposed by solution of borax in glycerin, and there can be little doubt that the reaction is the same—that is to say, that the glycerin separates the biborate into free boracic acid and a more basic borate. This more basic borate is probably not strongly alkaline in glycerin—at least our experiments show that while sodium biborate is acid in glycerin, sodium monoborate is alkaline.—(*Pharm. Journ.*, April 13.)

(b) *Subnitrate of Bismuth and the Alkaline Bicarbonates*.—Two theories have been advanced to explain the incompatibility which exists between these compounds—one, that the evolution of CO_2 from the bicarbonate is due to free acid in the bismuth subnitrate; the other, that there is a mutual decomposition between the two salts. The following rough experiments, performed by Mr. T. Green, seem to bear out the latter assumption:—Two drachms of bismuth subnitrate and the same of sodium bicarbonate were mixed with a small quantity of distilled water, and the bottle was corked and set aside. In a few minutes effervescence commenced, and in about an hour the cork was expelled from the bottle. The cork was replaced, and the reaction allowed to go on until all effervescence had ceased. The mixture was then transferred to a small filter, and the filtrate tested for nitrates, evidence of which was found. The precipitate, after being very well washed, was also examined for nitrates, but without success. It, however, effervesced briskly on the addition of dilute sulphuric acid, proving the presence of CO_2 . Examined by a lens, the precipitate had lost the crystalline structure of the subnitrate, corresponding now in appearance with the carbonate. This experiment was repeated, substituting sodium carbonate for the bicarbonate, with the following result:—No effervescence whatever took place, but at the end of forty-eight hours the bismuth was examined, and found to be entirely converted into the carbonate. The bismuth subnitrate was perfectly neutral in its action on litmus paper, as the latter experiment fully proves. It would thus appear that independently of any free acid that may be present in subnitrate of bismuth, owing to insufficient washing, (1) that an admixture of this bismuth salt with the alkaline carbonates or bicarbonates results in mutual decomposition; (2) that when the carbonates are used, decomposition without effervescence ensues; and (3) that when the bicarbonates are used, decomposition with liberation of CO_2 takes place. The following equations will render these propositions intelligible:—



(*Pharm. Journ.*, Dec. 21.)

PART IV.
MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL
SOCIETY.

FORTY-FIRST ANNUAL SESSION.

EDWARD B. SINCLAIR, M.D., President.
WILLIAM ROE, M.D., Honorary Secretary.

Saturday, February 1st, 1879.

E. B. SINCLAIR, M.D., President, in the Chair.

Ovarian Tumour.

DR. KIDD.—I wish to lay before the Society an ovarian tumour I removed from a lady, aged thirty-nine years, on Thursday last. There is nothing whatever new about the case. My chief object is to record a case that, I am sorry to say, ended fatally. The operation was performed at eleven o'clock on Thursday, and the patient died about two o'clock on Saturday morning. It was recognised before operation as a multilocular tumour, with a large hard mass lying in the left iliac fossa. On opening the abdomen patches of adhesion were found in the right hypochondrium. The first cyst tapped gave exit to a quantity of clear straw-coloured fluid, a second to a large quantity of dark-brown fluid thicker than serum, but not gelatinous. The cysts were so numerous that I found tapping them utterly useless, and for that reason enlarged the opening, passed in my hand, and broke down the cysts with my fingers. There was no difficulty in removing the tumour, except as regarded the one limited patch of adhesion. The most interesting feature in the case was the long duration of it. More than ten years had elapsed since I recognised the existence of it, and I had had the patient under observation during that period. At that time she was in exceedingly bad health. She had been confined to bed for several years, but from causes in no way connected with the ovarian tumour. When she first came under my observation the tumour was so small and deeply seated in the abdomen

that the nature of it was recognisable with difficulty. My first impression was that it was probably a faecal accumulation. On my second visit to the lady, who was at that time residing in the neighbourhood of Black-rock, the nature of the tumour was recognised. She recovered from the condition she was then in, and began to enjoy life, and, instead of being bed-ridden, was able to walk from one end of Dublin to the other. She improved in appearance, and in her constitutional health generally. The tumour, however, continued growing, but very slowly, until a few months ago, when it began to increase very rapidly. She got an attack of localised peritonitis in the right hypochondriac region about two years ago, which was the cause of the patch of adhesion which you see. She suffered great pain, and had a very considerable amount of fever. Even then the tumour was not so large as to make an operation imperative. The peritonitis yielded to treatment, and she spent the summer before last at Dalkey, and was comparatively well. When I saw her again I found the tumour considerably enlarged. It was interfering so much with her digestion that her food could not be retained, and she was growing weak and emaciated. I at once saw that the time had come for the removal of the tumour, and recommended it to be done. In cases of ovarian disease the important consideration is whether the tumour is of such a character as to imperil the life of the patient. I have a very strong feeling myself that the operation is of so dangerous a character that it should not be attempted unless the patient is in danger of dying from the disease. I think this case, though it turned out unfortunately, rather confirms the opinion that I have long held. The tumour had existed for ten years, and during that period the lady had enjoyed comparative health; and yet she died in forty-eight hours after the operation. Whether or not she would have died if the operation had been performed earlier, is an open question. We know that even under the most favourable circumstances the operation is attended with enormous risk.

THE PRESIDENT.—What was the amount of fluid removed by the tappings?

DR. KIDD.—Seven quarts and a pint.

THE HON. SECRETARY read the following paper:—

Case of Ovariectomy. By **WILLIAM HUME HART**, M.D., M.Ch., Colonial Surgeon, Sierra Leone.

BETSY WRIGHT, aged fifty-six, widow, admitted into Colonial Hospital, Sierra Leone, November 8th, 1878. She came to the dispensary complaining of a large swelling in the abdomen, for which she had tried all kinds of native remedies without avail, and had resigned herself to her fate, when her friends persuaded her to put her case in my hands, soon after my arrival in the colony.

She states that six years ago she first noticed a swelling, the size of a

hen's egg, in the right iliac region, which caused her severe pain, especially at the menstrual periods. This increased gradually in size, spreading in all directions, so as entirely to fill the abdomen. Four years ago the menses ceased altogether, and from that time she has felt no pain, though she suffers greatly from the effects of the enormous distension the abdomen has undergone, and the weight of the tumour she has to carry. Obstinate constipation causes great discomfort, her bowels being only moved by powerful purgatives. There was no symptoms of bladder irritation. On examination, the tumour was found to be very large, firm and multilocular, as evidenced by the localised fluctuation not being perceptible in any part beyond a space of 3 inches diameter. The uterus was normal for a woman of her time of life, and there was no apparent disease of any other organs except the right ovary. Her general aspect is that of a healthy woman, slightly emaciated; and after she had learned of even the chance of a cure, at any risk, she became quite cheerful.

On admission, the measurements were as follows:—Round abdomen at level of umbilicus, 40½ inches; from symphysis pubis to umbilicus, 11 inches; from umbilicus to ensiform cartilage, 11 inches. I put her on full diet, with chalybeate cathartics; and after making several exhaustive examinations alone, and with my assistant, Dr. Robert Smith, I decided on performing ovariectomy, as giving her the only possible chance of a comfortable existence. She was particularly anxious for it herself, notwithstanding all risks.

The Operation.—On Sunday, the 1st December, 1878, after clearing the rectum by a salt-water enema, she was placed on the table at 9 a.m. in a balcony, outside the room prepared for her reception, and carefully isolated from the wards. Dr. Colthurst, of H.M.S. Seagull, and all the civil practitioners of the colony, were present. As soon as she was thoroughly under the influence of chloroform the operation was commenced. I made an incision, from just below the umbilicus, in the middle line, to within an inch of the symphysis pubis, dividing, in the linea alba, all the layers superjacent to the peritoneum in order, and the peritoneum last, on a director. There was no hæmorrhage so far, and only a little serum escaped from the peritoneal cavity. Now the difficulty of the case began to present itself, the tumour being still immovable and closely covered by the omentum, which contained many enlarged veins. On plunging the trocar into the largest cyst, as previously defined, only a little bloody serum welled out, the structure of the tumour being too firm to admit of its collapse.

I accordingly enlarged the opening, extending the incision gradually to within 3 inches of the ensiform cartilage, when at last the tumour protruded, and I was able to examine it more thoroughly, and discover its alarming features. Covering it completely in front and on each side was the omentum of the lesser intestine stretched tightly over the tumour, and

adherent in many patches. I at once commenced its removal, leaving many of the patches attached, and ligaturing and dividing the vessels which were much enlarged. So far there was little hæmorrhage; the amount of time and consequent exposure involved being the most apparent danger. But now the real danger of the case presented itself, for the omentum being freed up to its attachment to the intestine, this itself was found in the left lumbar region to be closely adherent to the wall of the tumour for over 3 feet of its length, so close that it was a matter of considerable difficulty to dissect it up without injuring its structure, save at the expense of the tumour itself; and the latter being so vascular that each scrape of the scalpel caused copious uncontrollable hæmorrhage. I stopped the dissection, and at once set about ligaturing the pedicle; this was short and thick, and required three double ligatures before it was completely commanded. I then returned to the dissection, but almost unavoidably made an incision in the intestine itself, which involved a few sutures when the whole was separated. I then secured the pedicle and applied the actual cautery to the divided surface, as well as to many of the divided omental vessels.

Her strength was failing fast—the pulse being almost imperceptible—so stimulants were employed, and the necessary cleansing and closure of the wound, leaving the pedicle inside, were completed as quickly as possible.

The whole operation lasted 65 minutes, and immediately it was over she was removed to her bed (the room heated with a stove), and had turpentine enema administered, ammonia to nostrils, hot blankets and hot water bottles to (if possible) restore animation—but all to no purpose, as she never recovered consciousness, and gradually sunk, notwithstanding subcutaneous injection of ether, and, finally, artificial respiration, till she ceased to breathe and the heart to beat, within an hour from the completion of the operation.

The tumour preserved its ovoid shape, and when cut into showed dense fibrous structures mixed with adipose tissue, and chambered into numerous loculi varying in size from a pigeon's to a swan's egg. These cysts were each filled with serum, of exactly the colour of venous blood, and appeared like venous enlargements, bleeding as freely when opened, during the operation, as the veins themselves. From these cysts there drained, in addition to what escaped during the operation, ninety ounces (90 ozs.) of bloody fluid; and after all had drained away the solid mass weighed (24½ lbs.) twenty-four and a half pounds, being upwards of 30 pounds in all.

Is it possible to diagnose during life the presence of adhesions of the intestines themselves?

Was the amount of blood lost sufficient to cause death, being only part of what was in the tumour itself when it was ligatured? or—

Was death the result of the prolonged exposure?

DR. MACAN.—The questions of hæmorrhage from the adhesions and the treatment adopted by Dr. Hart are very interesting. The difficulty is that the hæmorrhage sometimes comes from the other side of the adhesions, and, consequently, to ligature the pedicle is of no use. The tumour seems to have been of a very unpleasant nature. Dr. Hart does not seem to have followed Dr. Kidd's plan of making a large incision and endeavouring to scoop out the contents. As to whether or not the woman died from the hæmorrhage it is very hard for us to say, when we do not know the exact extent of it. As to the duration of the operation, it has sometimes lasted very much longer than an hour and five minutes. Therefore I do not think we have grounds for attributing her death to anything except the shock. Did he sew up the intestine?

DR. ROE.—He applied sutures.

DR. MACAN.—I do not think anyone has yet attempted to diagnose the existence of adhesions between a tumour and the intestines. Adhesions in front, or pelvic adhesions, can, of course, be diagnosed.

DR. KIDD.—I once diagnosed the adhesion of an ovarian tumour to the omentum. An operation was performed by Dr. Barton, at the Adelaide Hospital. He allowed me to see it. Before the operation I warned him that he had the omentum adherent to the front of the tumour. There was an umbilical hernia. The invariable rule is that when there is no adhesion the omentum is pushed up along with the intestines, and the tumour is directly in contact with the parietal layer of the peritoneum. In this case we had an umbilical hernia which, from its feel and dull sound on percussion, we had no difficulty in recognising as an omental hernia; and the inference seemed to be that the omentum must be tied down some way in front of the tumour when it was possible for a portion of it to escape through the umbilical opening. It was a trivial matter, but the diagnosis turned out to be correct. A very important point is in reference to adhesions to the intestines. All our best authorities on the subject lay down the rule that where you have close adhesions to the intestines, it is better to cut away the tumour, leaving a portion of it adherent to the intestine. In a case which occurred in my own practice, and which I hope to bring forward, we found a very close adhesion between the tumour and a portion of the small intestine—so close that I thought it would be unsafe to attempt to detach it. I cut away a portion of the cyst wall, and left it adherent to the intestine; and I am happy to say that the patient recovered, although every portion of the tumour was closely adherent. The adhesions were exceedingly numerous. There was one strong thick band low down on the left side of the pelvis, running nearly from the mesial line, along the brim of the pelvis, and towards the broad ligament. It was so thick that I ligatured it. Altogether I left seven ligatures in the abdomen. After the clamp separated serous fluid began to exude from the stump and site of the

wound. For months afterwards there was a sinus at the wound, through which quantities of serous fluid exuded. Our first impression was that it was urine, and I confess I was very apprehensive that, in dividing the strong band on the left side of the pelvis, I had cut across the ureter; however, the sinus closed, and the patient regained perfect health without any uræmic symptoms, so that I was convinced that I did not do so. I believe the most reasonable explanation of the fact was that the portion of the cyst left adherent to the intestine contained a secreting surface, and threw out this serous fluid, which made its way through the wound.

DR. MORE MADDEN.—One remarkable circumstance about Dr. Kidd's case is the length of time this woman lived with the tumour. Some time ago I removed an ovarian tumour from a lady considerably over seventy years of age. The operation was unsuccessful, and she died. It was performed simply to relieve very urgent and pressing symptoms, for she had lived for some thirty years with the tumour; and the probability is that if she had been operated on at an earlier period she would have died in eight or ten hours afterwards.

DR. HENRY KENNEDY.—As regards the duration of these tumours I would refer the meeting to two authorities. One is Bright's famous volume on abdominal tumours, and the other is a paper by the late Abraham Colles, who mentions that he used mercury to relieve the great suffering entailed by the tumour.

DR. KIDD (in reply).—We know that many ovarian cases have lasted very long, and one is recorded of a woman who was tapped 107 times. That, however, is not the rule, but the exception. I believe that in the large majority of cases the disease runs its course in from three to four years. That three or four years is usually a life of misery, and the death is the most painful that a woman could die, or that a man could look upon. I have seen patients die from ovarian disease, and I must say the most painful duty I have ever had to perform was that of seeing them in their later days.

On Contracted Pelvis. By ARTHUR V. MACAN, M.B., &c.

THERE is in the whole range of obstetrics probably no problem on the practical solution of which such momentous issues depend as the determination, in any given case, whether pelvic deformity be present, and, if so, what is its nature and extent. There is also, I fear, no subject which to the student seems surrounded with such impenetrable mystery, or from which he turns more thoroughly confused and disheartened, after having consulted all the most celebrated text-books within his reach. For, though the importance of the subject is admitted by all authors, and whole chapters devoted to a minute description of the causes and results of rachitis or malacosteon, and accurate measurements and plates given of the deformities known as the masculine,

the oblique, the kyphotic, the spondylolisthetic pelvis, and many others, still the methods that are given for the solution in any given case of the question—"Is this pelvis deformed, and if it be, what is the nature and extent of the deformity?"—are so erroneous in principle, and found so difficult in practice, that in ninety-nine cases out of a hundred the deformity is first discovered by the delay which it causes in labour, and the amount of it is determined according as the labour is terminated by the forceps, the perforator, or the cephalotribe. I do not, of course, mean to say that in a Society such as I have the honour of addressing to-night, there are not many men who have learned by their own accurate observation and large experience to estimate very correctly the presence and the extent of pelvic deformity, but what I do mean is, that they are the exceptions rather than the rule, and are certainly not indebted for their skill to the assistance they may have obtained from any of the ordinary English text-books on the subject.

Let us begin by consulting the justly celebrated work of Ramsbotham. We find it therein laid down that there is little use in making any external measurements of the pelvis; for, though in a normal pelvis deductions about the internal diameters may perhaps be drawn from the measurements of the external ones, in cases of deformity such measurements "afford the most conflicting and erroneous results" (5th Ed., p. 27). For measuring the internal diameters of the pelvis, Ramsbotham prefers the hand to any other pelvimeter. "Three methods are," he says, "practised; one is by the introduction of the first finger of the right hand within the vagina, so that the point should be carried up to and touch the sacral promontory, while the root of the finger is applied exactly under the symphysis pubis, at the upper part of the arch. It must be evident that this mode of inquiry will be of no avail unless the pelvis be greatly distorted—*considerably under three inches*, indeed, in the conjugate diameter—for the ordinary length of the index finger along its inner edge is less than three inches; and as the oblique line from the promontory to the apex of the pubic arch exceeds the direct line across, so, if there be more than the space just mentioned, the finger would not be able to reach the projection, and we should consequently be in utter ignorance what amount of room existed. If the pelvis be very small, the sacral promontory can be felt with ease; but, even in that case, the dimension of the direct conjugate diameter is not afforded, but the length of the oblique line is given; *and it is not always possible to calculate the difference between these two lines accurately*" (5th Ed., p. 28). The second method is by the introduction of the whole hand into the vagina, and seeing how many fingers will enter the brim. The third method he considers the best, and it is the one he himself adopts:—"Two fingers of the left hand are to be carried within the vagina; the extremity of the first finger is to be placed exactly behind the symphysis pubis, and the tip of the

second against the sacral promontory (*vide* Fig. 147). By stretching the fingers in this way we shall have little difficulty in reaching the promontory of the sacrum, *even when the pelvis is of ordinary dimensions*; and by withdrawing them in the same position we may measure off the distance between their extremities on the first finger of the right hand, or on a scale of inches, or with the limbs of a pair of compasses; and, consequently, we arrive at an accurate knowledge of the exact dimensions of the pelvic brim. The laxity of the vagina and other soft structures, which almost invariably attends the process of labour, will permit the fingers to be withdrawn while extended, and if the examiner use sufficient care, they may be kept perfectly steady until the space which they embrace be ascertained. This mode of proceeding possesses a great advantage over the other two, inasmuch as we are equally well able to make our examination whether the head be occupying a part of the pelvic cavity, or whether it be still detained quite above the brim; for, even if it be engaged in the vagina, one finger may be passed anterior to, and the other behind it, with comparative ease."

What help have we obtained from all this? We have been saved the trouble of making any external examination, and infer that the first method is very easy, but only applicable when the distance between the lower margin of the arch of the pubis and the promontory—or, as it is now generally called, the diagonal conjugate—measures three inches or under, and, consequently, the true conjugate "considerably under" three inches. If this be true, since three inches is almost universally acknowledged as being the smallest conjugate through which a living child can pass at the full time, it at once follows that if we can touch the promontory with the tip of the first finger inserted into the vagina there is no chance of the child being born alive, and we should therefore perforate as soon as the os is fully dilated.

Let us now pass on to the third method, leaving the second to be considered last. This is what Ramsbotham considers the best, and is what he himself practises. It is with the utmost diffidence that I venture to differ from so high an authority; but, though it is the method proposed by Velpeau, and has found favour with a great many other high authorities, it seems to me to be thoroughly unreliable from the extreme difficulty of putting it into practice. Except in cases of great distortion it must necessitate the introduction of the whole hand into the vagina, and it is quite impossible, I think, to withdraw the fingers so accurately in the position they occupied at the brim of the pelvis as to exclude the possibility of a very serious error. Velpeau proposed the insertion of the fingers of the right hand in the angle between the extended first and second fingers of the left, so as to prevent them being approximated by the pressure of the soft parts; but even this manœuvre will hardly make the method accurate, and as it practically requires the whole hand to be

introduced into the vagina, it is just as painful for the patient as the second method, without having the advantage of being accurate.

What is then the objection to the second method? It is that it would necessitate the introduction of the whole hand into the vagina at a very early period of every labour, whether there was deformity present or not; for otherwise we could not diagnose the deformity till made apparent by the delay in labour, when it would probably be too late to turn with the view of saving the child. But patients would never consent to this, and hence this second method will never come into common use. We are thus left without any practical method of estimating the amount of the deformity except in cases where the true conjugate happens to be "considerably under" three inches.

If we now consult Murphy's "*Midwifery*" we will find exactly the same theory laid down with regard to the measurement of the true conjugate, except that he has not overlooked the possibility of making the measurement with two fingers instead of one. He says—"If one or two fingers be pressed towards the promontory of the sacrum, and they at all approach it, it is certain the promontory projects too much; for otherwise this could never happen" (2nd Ed., p. 168). "In more doubtful cases" he introduces the whole hand into the vagina. He agrees with Ramsbotham that the results obtained by external measurement are quite unreliable, while he does not so much as mention the third method, from which we may conclude that he does not think it very valuable.

If now, passing on to more recent times, we consult Tyler Smith, we will not find that he gives us very much additional information. He, too, discards the external measurement of the pelvis, except in the case of the obliquely contracted pelvis of Prof. Nægele, and looks on the finger as the best pelvimeter. He says:—"Before the coming on of labour, deformities of the pelvic *brim* may sometimes be measured by introducing the two first fingers, and separating them so as to touch the promontory of the sacrum and the symphysis pubis, and then altering their position so as to measure the transverse diameter. When the deformity is high up, this manipulation fails, and it becomes necessary to introduce the whole hand into the vagina, when a satisfactory exploration can be made. . . . The single finger passed in to its utmost length and rotated within the pelvis will seldom fail to inform us of the existence of deformity or diseased growths in the pelvis" (p. 426).

Thus he adopts Ramsbotham's third method even for the measurement of the transverse diameter of the brim, but thinks that deformity should generally be recognised by the examination with one finger.

Dr. Churchill gives the following directions for diagnosing pelvic deformity:—"When making an examination for this purpose, the finger should be passed direct to the promontory of the sacrum, and thence carried forward slowly to the symphysis pubis; we may then pass it

across the pelvis, in the direction of the transverse and oblique diameters, and finally follow the course of the brim, taking note of any deviation from the usual form, or of any obstacle" ("Midwifery," 6th Ed., p. 22). He admits, however, that the information thus obtained is "deficient in precision," which it could hardly be if it were possible to carry out this method in practice. He is the only one of the older writers who does not utterly discard the external measurements, which he thinks are "of some value as an approximative estimate" of the internal ones (p. 21).

Dr. Meadows thinks that in cases of slight distortion examination with the finger is practically useless, "inasmuch as the finger will probably not be long enough to reach the sacral promontory at all." "If the top of the sacrum can be reached easily with the finger there is probably coarctation of the brim; its absence, however, is no proof of the contrary" (*sic*), p. 331. He, too, omits all mention of the external measurements as aiding our diagnosis, and condemns the examination with the whole hand as being clumsy, painful, and untrustworthy, and finally adopts Ramsbotham's third method (3rd Ed., p. 338).

Dr. Playfair, in his late work, has shaken himself free from the old idea that the external measurements of the pelvis are of little value, and quotes the deductions that Professor Spiegelberg draws from a comparison of the distance between the anterior superior spines of the ilia with that between the two crests. He still adheres, however, to the old teaching about the impossibility in a normal pelvis of reaching the promontory without introducing the hand into the vagina; he fails to point out what are the conditions that determine the relative length of the conjugata diagonalis and the conjugata vera, while he directs the examination to be made with the right hand, the woman being in the ordinary obstetric position on the left side, under which conditions accuracy is practically impossible. His directions are as follows:—"The patient lying in the usual obstetric position, an attempt is made to reach the promontory of the sacrum with the tip of the index finger. In a healthy pelvis this is impossible, so that the mere fact of one being able to do so proves the existence of contraction. A mark is made with the nail of the index of the left hand on that part of the examining finger which rests under the symphysis, and then the distance from this to the tip of the finger, *less half an inch*, may be taken to indicate the measurement of the true conjugate of the brim" (p. 67). He adds, further on—"It is to be remembered that this procedure is useless in the slightest degrees of contraction in which the promontory of the sacrum cannot be reached" (p. 68).

If we now consult the masterly work of Prof. Leishman we will find that he contents himself by quoting at length the three methods of Ramsbotham. He adds:—"It is only, however, after considerable experience that such arbitrary methods of examination are of much value

in diagnosis. Very marked deformity is usually recognised easily enough, but the more important question of the degree or amount of the distortion is not so readily solved, and will always require most careful and exact observation. It is upon the latter, indeed, that the most important practical considerations hinge; and upon the result of such an investigation, be it right or wrong, will depend whether, in a given case, we determine in favour of operation by the forceps, turning, craniotomy, or the Cæsarean section."

Finally, if we consult Dr. Milne's "Midwifery," which is the latest book that has appeared on the subject, we will find that he too adopts Ramsbotham's third method as the best for measuring the true conjugate. If this fails—which, from his own account, it seems to do pretty often—he introduces the whole hand. He does not, however, as much as mention the first method of Ramsbotham ("Midwifery," 2nd Ed., 1879, p. 12).

One thing, I think, is very apparent from the foregoing extracts—viz., that the directions given in English text-books as to the best method of estimating deformities of the pelvis are most contradictory and confusing. What one writer looks on as the best method another does not even mention; one considers the external measurements of the pelvis as most valuable, while another speaks of them as "affording the most conflicting and erroneous results." Dr. Murphy looks on the introduction of the whole hand into the vagina as entirely clearing up a doubtful case; Dr. Meadows speaks of it as being "clumsy, painful, and untrustworthy." There is one point, however, on which they all agree—viz., that in the normal pelvis the promontory of the sacrum cannot be reached by introducing one, or (when it is mentioned at all) even two, fingers into the vagina. This statement is certainly untrue, but it is not nearly so dangerous as the one made by Ramsbotham and endorsed by Leishman—that when we are able to touch the promontory with the finger, the true conjugate must measure "considerably under three inches," or, as Murphy puts it, must be "less" than three inches. The last author, however, does not hesitate to draw most important practical conclusions from the fact. He says:—"It may then be taken as a rule, that if the forefinger touch near the promontory of the sacrum, *the head will not pass*" (2nd Ed., p. 258). The other writers who recommend this first method of examination either do not attempt to fix the extent of deformity which must be present if the sacro-vertebral angle is within reach of the finger, or express it in the most vague and general terms; and no one except Dr. Playfair attempts to estimate the amount of the difference between the conjugata diagonalis and the conjugata vera.

As long as authors differ so widely as this in theory, it naturally follows that there can be little or no unanimity as to any practice which is based or founded on the supposed length of the antero-posterior diameter of the brim, whether it be the question of turning or the forceps in contracted

pelvis, or the period of pregnancy at which premature labour should be induced, or the exact limits of craniotomy and the Cæsarean section. Thus, Dr. Murphy would at once make up his mind to perforate if he were able to touch the promontory with the finger, though fortunately very few of us would feel inclined to follow his example. Dr. Meadows would expect to deliver by turning, at the seventh month, in a case where the antero-posterior diameter of the brim measured $1\frac{3}{4}$ inches only, while Professor Spiegelberg would fix the lowest limits of craniotomy at 5.5 cm., or more than 2 inches. Dr. Playfair adopts a method of estimating the true conjugate from the diagonal conjugate, which leaves room for an error to creep in of considerably more than $\frac{1}{2}$ an inch, while in the same chapter he recommends a table for our guidance as to the week in which premature labour should be induced in cases of deformity, which presupposes an accuracy in the measurement of the true conjugate to within from $\frac{1}{8}$ to $\frac{1}{4}$ of an inch.

Such differences of opinion about various modes of treatment in cases of contracted pelvis would be very much more common than they are, were it not that a large number—I would almost say an immense majority—of the profession, never attempt to treat a case of difficult labour by estimating the exact length of the antero-posterior diameter of the brim, but determine the amount of the deformity in each case by the extent of the delay in labour, and the nature of the operation—whether forceps, or craniotomy, or the Cæsarean section—that is found necessary to end it.

The question of the time for inducing premature labour is also settled, in the vast majority of cases, by the history of the previous labours, and very rarely indeed, in spite of the table recommended for our guidance by Dr. Playfair, by any exact measurement of the true conjugate.

But what have we learned from these authors as to the relative lengths of the diagonal conjugate and the true conjugate? Murphy contents himself with showing that the true conjugate must be “less” than the diagonal. Ramsbotham and Leishman say that it is “considerably less,” but add that, even in cases of great deformity, “it is not always possible to calculate the difference between the two lines accurately.” Dr. Playfair simply deducts half an inch from the diagonal, and says the amount thus obtained “may be taken to indicate the measurement of the true conjugate of the brim” (Vol. II., p. 68). This leaves room for a very considerable error to creep in, as the difference between the two conjugates varies greatly in different cases (1–3 cm.).

The general teaching of the present day, with regard to deformity of the pelvis, may, I think, be thus shortly summed up:—

First.—That external measurements of the pelvis are almost useless.

Secondly.—That the promontory of the sacrum cannot be reached by the finger except in cases where the true conjugate is either “less,” or “considerably less,” than three inches.

Thirdly.—That it is therefore impossible, by this method, to estimate the amount of the deformity when the diagonal conjugate measures more than three inches.

Fourthly.—That, even if we know the length of the diagonal conjugate, we cannot, with any degree of accuracy, estimate from it the length of the true conjugate.

Fifthly.—That in order to estimate the deformity, in cases where the diagonal conjugate is more than three inches, we must either introduce the whole hand, or measure the brim directly by Ramsbotham's third method of placing one finger on the pubis and the other on the promontory, then withdrawing them without altering the distance between the points, and measuring this off on a rule or by compasses.

It is, I think, very humiliating to find, if we consult the works of Smellie, more especially his first collection of cases, published in 1754, or nearly 130 years ago, that his practice in cases of pelvic deformity was founded on sounder theory and more exact observation than that of the present day. He did not think that if the promontory was within reach of the finger the true conjugate must be considerably less than 3 inches, and the child, therefore, be inevitably lost, as may be seen by the following case (Smellie, Vol. II., p. 261, case 192, M'Clintock's Edition):—"In the year 1742, my attendance was bespoken to a woman who had been four times delivered by another gentleman of dead children; and it was alleged her pelvis was so narrow and illformed, that she could not bear a live child. . . . Soon after my first visit, I was called to her when she imagined herself in labour, and found the mouth of the womb but very little open, though soft and yielding. Her pains seemed to proceed from her being costive; yet I felt the head resting above the pubes, and I was agreeably surprised to find the pelvis was not so narrow as it had been described; for with the tip of my finger I could hardly touch the jutting forwards of the last vertebra of the loins and upper part of the sacrum; from which circumstance I understood the pelvis at that part was not above half or three-quarters of an inch narrower than those that are well formed. I therefore hoped that if the child was not large it might be saved, provided I could keep up the patient's strength." Smellie estimated the true conjugate in this case as being $\frac{1}{2}$ or $\frac{3}{4}$ of an inch less than normal—or from $3\frac{1}{2}$ to $3\frac{3}{4}$ inches, gross measurement—and that this was a very accurate estimate is shown by the fact that the child was born alive after a moderately tedious labour, so that he adds—"From the easiness of the birth and the round form of the head, which was not at all compressed, I am inclined to believe that though the child had been of an ordinary size it could have been saved." To him, also, the great credit is due of being the first who attempted to estimate the true conjugate from the length of the diagonal; and I think there is more accurate information and practical knowledge to be derived

by reading the two short chapters in which he treats "Of the Narrow and Distorted Pelvis," and the cases he gives of labour with contracted pelvis, than in all the modern English text-books put together. It must also be greatly regretted that instead of being the teachers, as the English were in the time of Smellie, we are now the taught, and have not even proved ourselves apt scholars. For it is now nearly 30 years since Michaelis' celebrated work on deformities of the pelvis ("*Das Enge Becken*") was published, in which, after pointing out the ignorance that then prevailed in Germany on the subject, he says:—"In order not to continue on in this strain longer than is absolutely necessary to prove my point, I will instance one error only, which, as far as I can ascertain, is to be found in all the text-books, and the tendency of which must be to prevent any systematic measurement of the pelvis—viz., the statement that it is impossible in a normal pelvis to reach the promontory of the sacrum by introducing one or two fingers into the vagina. *This statement is absolutely untrue*, for it is very rarely, indeed, that we fail not only to touch the promontory of the sacrum, but also to measure accurately the diagonal conjugate. That this may in exceptional cases be impossible is no more than one would expect, but that these cases are very rare may be gathered from the fact that I have been able to make this measurement in fifty consecutive cases without once failing, and can generally measure it in nine cases out of ten" (2nd Ed., p. 8). Since the publication of this book this error has quite disappeared from German text-books, and the new teaching has even been adopted by the French, who are not in general very ready to receive any foreign ideas. If we look into Cazeaux's book we will find he considers that in the large majority of cases we can make a thoroughly satisfactory estimate of the length of the conjugata vera by introducing one or two fingers into the vagina; and that if we are unable to reach the sacro-vertebral angle there can be very little difficulty in labour (Cazeaux, 9th Ed., p. 681). The impetus that the work of Michaelis gave in Germany to the study of contraction of the pelvis has been fostered and kept up by such men as Litzmann, Dohrn, Schroeder, and many others; and the various scattered observations, unconnected facts, and half-formed theories thus obtained have been quite lately collected and woven into one harmonious whole by Prof. Spiegelberg, of Breslau ("*Lehrbuch der Geburtshülfe.*" 1878).

He looks on the external measurements of the pelvis as of the greatest importance as enabling us to determine to what type of deformed pelvis we are to refer any given case, though not the exact amount of deformity present. The external measurements that he thinks of most value are: that between the anterior superior spines of the ilia, that between the crests of the ilia, and the external antero-posterior diameter, or diameter of Baudelocque. In the normal pelvis the distance between the crests

is about one inch greater than that between the spines. He concludes that—

1. If these distances are both of average length, and the distance between the crests is about one inch greater than that between the spines, the pelvis is normal.

2. If the distances are both something under the normal, but retain the proper relative length, the pelvis is a generally contracted one.

3. If the distance between the crests is the average, while that between the spinous processes is more than usual, so as to be nearly equal to, or equal to, or even greater than that between the crests, then the pelvis is contracted in the antero-posterior diameter, but is normal in its other dimensions.

4. If the distances have the same relation to each other as in the last case, but are both below the average, then the pelvis is too small in all its diameters, but especially in the antero-posterior diameter.

He also says that should the external antero-posterior diameter be only 6.3 inches we may conclude that there is antero-posterior narrowing of the brim.

To measure the diagonal conjugate of the pelvis, the woman should lie on her back, with the hips well raised off the couch, in order not to prevent the finger and hand being pointed well upwards. The two first fingers are then to be inserted into the vagina, and the other fingers folded into the hollow of the hand. As soon as the external fingers come to touch the perinæum steady pressure must be made upwards and backwards towards the promontory. After a short time this overcomes the spasm of the perinæum, and in proportion as it becomes relaxed the fingers sink more deeply into the pelvis. When the tip of the second finger reaches the promontory the radial side of the first finger must be raised against the lower edge of the pubis, and a mark made with the nail of the forefinger of the other hand at the point where it touches the ligamentum arcuatum. The fingers are then withdrawn, and the distance measured off by a pelvimeter. This gives us the diagonal conjugate, which exceeds the true conjugate by from 1–3 cm., or roughly from $\frac{1}{2}$ –1 $\frac{1}{2}$ in. The exact amount must be determined in each case by taking into consideration the height of the promontory and the depth and inclination of the pubis. In this way Spiegelberg says he is able to arrive at the true length of the conjugate to within 2–4 mm., or from half a line to a line.

The most essential points to be attended to when measuring the diagonal are:—1st. That the promontory cannot be readily reached when the woman is lying on the left side, nor, if it be reached, can we then readily measure off the diagonal conjugate with the forefinger of the left hand. 2nd. That our success in reaching the promontory will depend as much on our wearing out the resistance offered to the hand by the

perinæum by constant firm pressure, as on the mere length of the fingers. 3rd. That the ease with which the promontory is reached depends greatly on its height, as well as on the length of the true conjugate.

As we neglected *for nearly 130 years* the teaching of our own celebrated countryman, Smellie, we can hardly be greatly astonished that the facts brought forward by Michaelis, now nearly thirty years ago, have had so little effect on the doctrines commonly taught in this country. But there are already symptoms of a change, and I have no doubt that before long the fatal theory that it is impossible to reach the sacro-vertebral angle in a normal pelvis, by inserting two fingers into the vagina, will be abandoned and forgotten in this country as it is at the present moment in Germany.

DR. KIDD.—Did Dr. Macan ever try Dr. Greenhalgh's ingenious instrument for measuring the pelvis? .

DR. MACAN.—I have not tried it.

DR. KIDD.—It sometimes answers the purpose very well, but measurement with the fingers is the best. My own plan is to introduce the right index finger and touch the promontory of the sacrum with its tip and the arch of the pubes with its radial side; then I introduce the left index along the palmar surface of the other till I touch the crest of the pubes, and withdrawing the hands in this position, the distance between the tips of the two fingers corresponds to the antero-posterior diameter of the brim. After all, the test is whether the child's head will come through. You may measure the pelvis, but you cannot measure the child's head. I attend a lady who was told she never could have a living child. I measured her pelvis, and said I thought I could deliver her of a living child. I delivered her of two—the first with the forceps and the second by turning. At her next labour I tried the forceps, but failed; I then turned, but had such difficulty in getting the head through the pelvis that the child was lost. It was an enormous child, weighing fourteen pounds. All her children were large.

DR. ROE.—I am in the habit of using a very simple appliance suggested by Mr. Weir, of the Coombe Hospital, and which consists of a steel ring made to fit any sized finger, which is fixed to the index finger; from this a cord passes to a small India-rubber band placed upon the second finger, which registers the exact distance to which the fingers have been separated in the pelvis. It can be measured off by a rule afterwards.

The PRESIDENT.—We know that, according to Baudelocque, in France every parish priest is advised to have a pair of callipers, and to recommend no person to marry that does not measure a certain amount round the pelvis.

The Society then adjourned.

THE DOCTOR
SOCIETY FOR
MEDICAL
OBSERVATION

TRANSACTIONS OF THE ULSTER MEDICAL SOCIETY.

SESSION 1878-79.

President—A. HARKIN, M.D., J.P.

Hon. Secretary—WILLIAM WHITLA, M.D.

Third Meeting.

A. HARKIN, M.D., President, in the Chair.

Discussion on the Milk-feeding of Infants at Nurse.

DR. WALES (ex-President) said: In opening the consideration of cow's milk feeding for infants, I will not go outside of the immediate question. With you we will, I am sure, mostly concur that milk as ordinary sold requires no dilution; but doubtless there will be differences in our experiences with regard to pure undiluted milk. Speaking for myself, I have frequently found it necessary to dilute milk both with water and lime-water to diminish the tendency to the formation of an indigestible coagulum even in children seven months of age. I certainly would not venture on ordering undiluted milk for a new born infant with my present ideas and experience. The irritation formed by milk coagulum must be familiar to most of us, and the difficulty of combating its consequences must be felt by all of us, as one of the trials of practice. Knowing that the casein of human milk forms light flocculi, while that of cow's milk coagulates in the infant's stomach into hard gelatinous pieces, often utterly indigestible, occasioning cholera infantum, I cannot think it wrong to reduce the proportion of casein and salts in the latter to at least the proportion existing in the former by the addition of water, and lime-water also if disposed to acidity. But the difficulty as to milk-feeding is, I think, satisfactorily solved by the introduction of Swiss condensed milk (1-9) which I regard as next to a good breast, and superior to the milk of one cow. I think when its value is generally appreciated it will reduce the mortality amongst bottle-fed infants immensely.

PROFESSOR DILL said that the President, in his opening address, had acknowledged the fact that cow's milk is stronger than that of the human female; and he would therefore dilute it with a little water, at the same time adding a small quantity of sugar, in which it is deficient. He was disposed to say that where the mother fails in nursing her own child we should fall back upon a wet nurse. In cases where bottle-feeding is adopted there is more than quality to be considered—there is quantity.

Some members of the profession had taken in hand to weigh a child before and after taking the breast milk, and found that from three to four ounces were taken at a time, amounting in all to about two pounds a day. Another important matter to be attended to is the temperature of the milk; it should have a mean temperature of 95° Fahrenheit. He considered this point quite as important as either quantity or quality, and thought that much injury arose from want of attention to it. He thought that if we could get over our prejudices, ass's milk would probably be the best as it comes nearer the mother's, but as society is constituted, we need not go beyond the cow's milk; but with all the care and attention which can be bestowed, it is a poor substitute for the soothing influence of a mother's lap and a mother's bosom; and no man of any experience could recommend bottle-feeding as compared with the breast. In consulting physiological chemists we find that the mother's milk varies much from first to last, becoming stronger as the child grows older, and is adapted to the child's strength up to that period when the child should be weaned. We should take a leaf from nature's book, and diminish dilution as the child advances. He would not dilute with barley-water, as some advocated, but use simply pure water. The cow's milk requires also a little alkali—depending on the state of the bowels—if loose, chalk or lime-water; if constipated, a little magnesia, and he considers Murray's solution of magnesia the best form.

DR. DEMPSEY said: Mr. President,—For the reasons which Dr. Wales has stated, with reference to the differences between the curd of woman's milk and cow's milk, I believe with him that cow's milk should be given diluted to infants in the early months of life; and it is for this reason that cow's milk is mixed with water when given to infants, and not merely because it is richer in solid constituents than woman's milk. I am in the habit of recommending the milk to be given diluted with one-third of water, and with the addition of a little sugar. Pavy recommends sugar of milk instead of ordinary sugar. He gives one ounce sugar of milk to three-quarters of a pint of milk, diluted with water in the proportion I have mentioned. I believe many of the cases of dyspepsia and marasmus seen in infants brought up by bottle-feeding to be due more to the carelessness of those who attend them than to the fact of diluting the milk. It is the usual practice of nurses to prepare a bottle-ful of milk for an infant's meal. Of course, one-half of this is not taken, and what is left is either kept warm or re-heated for the next meal, or probably mixed with fresh milk and re-heated a few times more. This system of re-heating sours and curdles the milk by converting the lactine into lactic acid. Sour milk is neither nutritious nor will it remain on the infant's stomach. Another cause of sickness and wasting in infants is the too early administration of farinaceous foods. Mothers believe that because older children thrive well on them, that, consequently,

they cannot be given too soon in life; but young infants, having no salivary secretion, cannot digest starchy food. If any food other than milk food is suitable for early infancy—the only one I can see to be chemically so is one prepared by Liebig. It is made by mixing wheaten flour, barley flour, a few grains bicarbonate of potash, water, and cow's milk, in stated proportions. This is made first by warming it and then boiling. The ferment in the malt, under warmth, converts the starch of both flours into dextrine and grape sugar, the latter of which gives the food a sweet taste. The bicarbonate of potash neutralises the acid reaction of the two kinds of flour and raises the alkalinity of the food to that of woman's milk. This food closely approaches woman's milk in chemical composition. With reference to the condensed milk which Dr. Wales recommends, it is quoted by Pavy that though a child fed on it appears to thrive well, and becomes plump and fat, this appearance is delusive, because under any exhausting disease, such as diarrhoea, it rapidly sinks. I think with care on the part of the nurses in having the bottles thoroughly clean and fresh, and in giving the milk in proper quantities, and at proper times, the mortality of bottle-fed children would be materially reduced. M. Guillot, the authority Dr. Dill referred to, from experiments conducted at the Foundling Hospital, Paris, calculated that a child under one month draws from the mother's breast from two to five ounces of milk at a time, and that two and a quarter pounds of milk is the smallest quantity that will suffice for the nourishment of a healthy child under one month old. I agree with the President that if the milk first taken from the cow be used, it may be given pure, but it contains very little cream, and is not so nutritious—the fatty matter rising in the gland of the cow, as it does out of it—the last of the milking being therefore the richest. For many reasons, when from some unavoidable cause the mother's milk is not available or suitable, personally, I would prefer, from my experience, hand-feeding, if carried out judiciously and carefully, to wet-nursing. There are so many influences at work—social as well as physiological—when a wet nurse is taken into a family, tending to derange the quality of her milk, and over which there can be little control, that I believe careful hand-feeding, judiciously attended to, is a more satisfactory and preferable way of rearing children.

DR. J. WALTON BROWNE had seen very satisfactory results on bottle-feeding with cow's milk—one-third water and a little sugar of milk added, up to nine months of age. He knew where barley-water was used, mixed with the milk, with the best results. With regard to the temperature of the milk, an American writer advocated giving the milk quite cold, which prevented dyspepsia.

DR. RIDDELL agreed with the President, and would go in for milk, pure and simple. Having practised in a country for three years, where for nine months out of the twelve cow's milk could not be procured, he

had seen children fed on Swiss milk, and found that the mortality was much greater than on cow's milk. He considered that the less sugar was added to the milk the better. The cow-keeper was in the habit of giving salines to increase the quantity of milk a cow would give, and this rendered it dilute. One corrective to colicky pains is to provide the child with some additional fat which is destroyed in heating the body. Half a teaspoonful of cod-liver oil, night and morning, makes a child thrive well. His own practice is to give milk in its purity, without warming. He considered wet nurses an abomination, and he very much preferred the bottle. He believed there would be fewer cases of rickets, and fewer little graves, if we confined ourselves to milk as it comes from the cow.

DR. M'CONNELL did not consider bottle-feeding and hand-feeding were at all synonymous. His experience at the union workhouse is that the bottles do not answer, as the nurses left the empty bottles and sour tubes for the child to suck, and he has therefore done away with bottles altogether. The plan he now adopts is to give an orphan child to one nurse's exclusive care, and he has increased the allowance of milk from a pint to a pint and a half, with one ounce flour and eight ounces bread, daily. There should be no fixed rule with regard to quantity, and he deals with each case on its merits, if it is at all peculiar; and with regard to quality there can be no rule, as the milk varies so much at different times and seasons. After the child is six months old he adds no water. The improvement in this system of spoon-feeding, without the use of bottles, has been very marked in the improvement of the children. He had observed that at the time of year when cows were getting large quantities of malt-grains the milk was rendered quite unfit for use, and that the same thing occurred where cows were fed on land which had been flooded—the milk turned quickly acid and stringy.

DR. CORE remarked that the question for the consideration of the meeting was not whether cow's milk was sufficiently diluted already before it reached the consumer, and therefore should not be further diluted, but whether pure cow's milk required any dilution to make it a suitable aliment for an infant from birth. He quite agreed with a remark made by Dr. Riddell, that a great evil arose from attempts to improve the work of the Creator; but as cow's milk was not the sustenance intended for an infant, he failed to see why the acceptance of that principle should prevent us, when the natural food was not available, making such changes in the cow's milk as would make it approach more nearly in character to the human milk. The fact that not only is the casein in cow's in a larger quantity than in human milk, but that the coagula formed by the former are large, hard, and indigestible, compared with those of the latter, seems to point unmistakably to the necessity for dilution. The amount of this must vary not only with the quality of

the milk, but with the age of the infant—the changes known to take place in mother's milk pointing out in this latter respect a line of practice which could not possibly be followed if pure milk were given from the first. He thought it very difficult, if not almost impossible, in a large town like Belfast, to get milk suitable for an infant, considering the ill-ventilated byres in which so many cows are kept, and the feeding which they get; he therefore agreed with Dr. Wales as to the superior excellence of Swiss condensed milk. He had used it with admirable results. It had one special advantage in that it was not liable to the acid changes which so commonly take place in cow's milk. Dr. Dempsey had quoted a remark of Dr. Pavy, to the effect that children fed on Swiss milk, though thriving well for a time, were liable to succumb to the first attack of serious disease. Now he (Dr. Core) had seen such children come well through serious disease. One case he remembered, especially from its apparent hopelessness at first, of an infant born prematurely, with whom nothing agreed—it was, in fact, dying. It was tried with Swiss milk, immediately began to thrive, and (though never robust) continued to do so. It afterwards came well through several attacks of exhausting disease. As to wet-nursing, he agreed with Dr. Dempsey in disliking it. The social and moral questions involved, the injustice done in most cases to the nurse's own infant, and the exceeding trouble that, as a rule, wet-nurses gave in a house, made him much prefer bottle-feeding, either with good cow's milk or with Swiss milk. He mentioned, incidentally, that he had seen a child fed on Nestle's milk-food in whom slight rickety symptoms had afterwards occurred, and asked whether any other member had any similar experience.

DR. LINDSAY believed that cases of marasmus occurred simply from the way in which the cattle were often fed, and considered it the duty of the medical attendant to see to it, in cases where children are bottle-fed, that the milk is pure.

DR. WORKMAN referred to some continental statistics, which proved that in the foundling hospitals the mortality was much greater where children were fed on the bottle than by wet nurses.

DR. ESLER said: Mr. President,—In a manufacturing community like Belfast, where so many thousands of the population are mill-workers, and many of these mill-workers mothers who must leave their children to be nursed by hand, it is alike important to the State—which puts a high value upon infantile life—and to the medical man, who, perhaps, puts upon it a higher estimate, that correct opinions should prevail, and, at the same time, be disseminated on this subject—of laying the foundation of a strong physical development for the next generation of men. In speaking on “the milk-feeding of infants,” I wish to refer first to one or two points in connexion with the natural food—the mother's milk.

A common practice prevails—and prevails extensively amongst the older nurses—that for a few days after birth the infant must be *fed* in order to keep it alive until the milk comes to the mother's breast, hence, toast and water, milk and water, or sugar and water is thrust down the infant's throat *ad libitum*; and in order to assist young humanity to get rid of this unnatural food, copious doses of castor-oil are administered at uncertain intervals. All this I hold to be wrong. I was very much struck with the practice of the native aboriginal tribes of Victoria, on several occasions, when I had ample opportunity of studying their habits. The pregnant female takes ill, the tribe halt on the march, the child is born, and, almost immediately, put to the breast; by the end of a day the mother, with her young infant swathed in a kangaroo's skin, is ready to rejoin the tribe. Following the teachings of Nature, I have been insisting on all my patients adopting the same plan—as far as the child is concerned—putting it to the breast at once, preventing the nurse giving it either food or oil. The advantages are double—the child has learned to suck by the time the flow of milk comes to the mammæ, the breast-milk acts as a natural purgative, there is no flatulence, colic, or purging, and the mother is, in almost every case, saved from that stage known as milk-fever. It is sometimes impossible, and in other cases not advisable that the mother should feed her infant. Where such a calamity occurs, the question the medical man has to decide is, whether the infant should have a wet nurse or be fed by the bottle. The selection of a wet nurse in most instances simply means a change of position for the two infants—the mother's own child is put upon the bottle, and the foster child on her breast. I have no hesitation in expressing my opinion that for the preservation of the life of a valuable child, I would certainly select a wet nurse; but the subject we have to deal with principally in this discussion is that of bottle-feeding by means of cow's milk or other artificial food. Those of our number who are connected with children's hospitals have ample opportunity of investigating this matter, and I think they will all agree with me in this statement—that *multitudes of our infantile population are starved to death by over-feeding*. I cannot better explain what I mean by this seemingly contradictory statement than by instancing a case which came before me only yesterday. One of that possibly (?) useful class of women who may be called baby-farmers, or nursing-mothers, brought a male child, aged four months, to hospital, with this history:—The child had been attended to by another woman until a month ago, when it was put in her charge; it was then rosy and healthy; she understood that it had got a quart of milk in the twenty-four hours. In order possibly to economise, her instructions were to give it a pint of milk daily, with plenty of arrowroot biscuits and sop. The child got diarrhœa, was wasting rapidly, and death stared us in the face. My instructions to the nurse were, to just continue her line of

feeding if she wished to see the child in its grave at no distant date; but that if its life was desired she must discontinue all feeding on solids, put the child on a quart of pure milk daily, with just enough boiling water added to make it milk-warm.

There can be no doubt, I think, from all one sees in practice, apart from chemical explanations, that starch as a food is not suited to children before they get their teeth. Cow's milk, and, if possible, milk from the same cow, with one-third water added to the average of the whole milking, with a little sugar of milk to make up for the natural deficiency in that ingredient, will be found in most cases to give satisfactory results for the first six months of infantile life.

Due attention must be paid to the state of the bottle and tubes. Where vomiting occurs it is generally the result of over-distension of the stomach. A diminution in the quantity given will rectify this, and in cases of acidity an alkali, such as the saccharated solution of lime, or, if indicated, magnesia may be prescribed and added to the milk.

Cases present themselves occasionally where, notwithstanding all care and attention, the child does not thrive on cow's milk. I have seen nearly all the artificial substitutes for cow's milk tried with varying success; but the most marked cases of benefit have resulted, in my experience, from prescribing Nestle's milk food as an addition to the ordinary milk in those obscure cases of wasting which do not come under any distinct heading, and which gives the doctor so much trouble and the parents so much anxiety.

Where you have a good constitution to work upon, bottle-feeding on cow's milk gives very general satisfaction; but where there is a delicate organism, or unfavourable surroundings, as in the crowded wards of the union workhouse, the best apparent care and attention is not sufficient to keep a bottle-fed child in life, especially in cold weather, where the natural heat of the mother's breast and bed are exchanged for the cold cot, and not very warm affection of the attendant nurse.

Our duty as advisers of the public weal in this matter I hold to be, to acquire right principles for our guidance, and, when opportunity offers, to disseminate these principles among those who can carry them into practice, so that at the same time the nurse may be instructed, the child's life saved, and our wisdom and reputation as preservers of human life placed in such a position, in connexion with the feeding of infants, that we may be recognised to be—what, indeed, medical men as a profession in a most eminent degree are—benefactors to our race.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

President—WILLIAM MOORE, M.D.

Secretary—E. H. BENNETT, M.D.

Hepatic Tumour Simulating Ovarian Disease.—DR. DUFFEY said: In the unavoidable absence of my colleague, Mr. O'Grady, he has requested me to bring forward this specimen. It is a liver which was removed from a woman, aged fifty, the mother of eleven children, the last of whom was born ten years ago. Catamenia ceased seven years since. She was admitted into Mercer's Hospital on the 28th of September, under the care of Mr. O'Grady, having, three months previously, observed a lump low down in the right iliac region. At that time it was about the size of a small cocoa-nut. On the 3rd of October she left the hospital. She returned on the 17th of the same month. There was a slight amount of ascites. The tumour was freely movable, and believed to be quite unconnected with the liver. After her second admission the ascites commenced to increase very rapidly to a large amount, and she suffered greatly from pain, which was of a vague character. She was extremely apathetic—a feature pointed out by Dr. James Little as of value in the diagnosis of malignant tumours from those of an aneurismal character. She was anxious, and repeatedly urged, that something should be done for her, even though she should die under the operation. I saw her only once—the day before her death—and with reference to the advisability of an operation. The abdomen was very much enlarged. On bringing the fingers suddenly down on it, in the right iliac region, they came upon a hard, nodulated, irregular mass. Mr. O'Grady thought there might be cancerous disease of the omentum, or in connexion with the intestines; at an early stage the question of fæcal accumulation arose, but on account of the change in the ascitic phenomena, an ovarian cyst was suspected. The uterus, on a physical examination, was found to be quite healthy. Her distress from the ascites was so great that it was decided to remove the fluid; and Mr. O'Grady thought that the best way of doing that was by making an incision in the abdomen, so that if it should turn out that there was a removable tumour, it might be operated on at once. A very large quantity of fluid was removed on the day before yesterday, by a small incision in the mesial line. It was of a clear colour and free from any flocculent deposit. On introducing the fingers, after the fluid had been removed, a large mass was found, which was at once recognised as being an hepatic tumour. There was a great deal of

hæmorrhage from the incision, which had been enlarged in order to let the fingers into the abdomen. The woman died in about twelve hours after the operation. There was a considerable amount of hæmorrhagic injection and thickening of the peritoneum, and there were also some clots in the peritoneal cavity, especially behind the spleen. The spleen itself was shrivelled up and of a slatish-gray colour, but apparently healthy, as is usual in these cases—this being, as is well known, a diagnostic point in distinguishing malignant disease of the liver from amyloid or cirrhotic enlargements of that organ, and in which cases the spleen is also found enlarged. A portion of the colon is adherent to the tumour. The disease occupies nearly the entire of the right lobe; and, although the liver was only removed yesterday, and has not yet been placed in any preservative fluid, the whole tissue is broken down and pulpy. Commencing below, there is a large tumour of, probably, a carcinomatous nature, about the size of a Seville orange. In some places it presents tumours resembling those sometimes described as fungus hæmatodes. I do not think the blood in the peritoneum was due to the bursting of any of these tumours; it is more likely to be due to weeping from the vessels of the peritoneum itself. There is another tumour, quite isolated, and of a harder consistence. The larger one is evidently that which we felt, and which was fully the size of a cocoa-nut. Elsewhere over the surface of the liver are numerous small, irregular growths. There were no enlarged glands, or any disease of the stomach, uterus, or kidneys. The gall-bladder was empty. The contrary is frequently the case, the presence of gall-stones being often noticed as the precursor of hepatic cancer. There was no jaundice, œdema of the body, or enlargement of the veins. The case ran the rapid course usual in such forms of disease—its whole duration being a little over five months.—*November 30, 1878.*

Dislocation of the Fifth Cervical Vertebra.—DR. E. H. BENNETT said: This is a specimen of injury of the spine of sufficient rarity to render it worthy of being brought before the Society. It is an example of dislocation of the spine; and, on looking through the Proceedings of the Society, I find only three similar cases recorded, the last being a very remarkable one brought forward by Dr. T. E. Little. The subject of this injury was a man aged forty-five years and of great size—I should say he was at least fifteen stone in weight. He was brought to Sir Patrick Dun's Hospital on the 3rd of July, at six o'clock in the afternoon. He had been sitting on a low area wall on City-quay, and, being drunk, suddenly fell backwards into the area, a height of six feet. When he was brought into the hospital it was difficult to determine the state of affairs, in consequence of his condition of intoxication. He was not only drunk at the time, but was an habitual drunkard. He was very fat and heavy, and had a great brawny neck, which made it extremely difficult

to ascertain the condition of the bones. I saw him a very short time after his admission. He was then vociferating and abusing the nurse, or anyone else who attempted to disturb him or to come near him. On those fits of excitement passing off, he lapsed into a comatose condition. He lay on his left side permanently, with his left hand beneath his head. There was no paralysis of the upper extremities or upper part of the trunk, but the lower limbs were profoundly paralysed, and there was incomplete priapism, and also retention of urine. Whether there was hyperæsthesia or anæsthesia it was impossible, on account of his condition, to determine. I attempted to place him on his back, in order to make an examination of his neck, but, by the power of his hands, he twisted his shoulders, and even his paralysed pelvis, over, and complained, in violent language, of being disturbed. We could make no examination of the posterior part of the neck, so as to ascertain whether or not there was any appreciable depression or deformity of the spine. It was easy to diagnose that there existed either a fracture or a dislocation of the spine, but what the exact diagnosis was was not so clear. We might, however, have attended more to the decubitus. His position, lying on the left side, with the head supported, was, I think, due to the fact that in this way only could he obtain relief from great pain. The explanation appears very clearly evident in the details of the specimen. The sequel of the case is remarkable. As it was apprehended that the bladder had become extremely distended, in consequence of the quantity of drink he had taken, it was emptied by means of a catheter. The man was then left without any further active treatment, and lay, apparently, in a state between coma and sleep for the greater part of the night. When I saw him in the morning at visiting hour, he was still in the same position. His left hand and arm lay outside the bed, and presented an appearance like that of a case of hæmorrhagic small-pox, there being an eruption and deep measly mottling, just like what is seen in extremely livid small-pox. He was only conscious enough to object to be stirred, muttering and struggling against any attempt to shift him. On stripping off his clothes, it was found that the greater part of the left side of the body had the same mottled appearance, although the markings were not so strong as on the upper and lower limbs. At the popliteal space on the left side there was a big patch, as large as the palm of my hand, discoloured and vesicated, containing coagulated serum. In the under part of the thigh a distinct crepitus was present. The limb was glazed, and the lower part of it was rapidly going into a condition of gangrene. There was emphysema of the areolar tissue along the whole of the left limb, so that the skin was completely glazed. I did not think the man's death so extremely imminent as it was. I left the ward and went into the next for a few minutes, and on my return again, I found that his face had become suffused, and that he was gasping for breath, with frothy lips, and in *articulo mortis*.

He died in three or four minutes afterwards. The extreme rapidity of his death was very unusual, especially the mode of it. Rapid deaths from broken necks are familiar to everybody; but in this case, where the man had been conscious and able to move, by the strength of his upper extremities, his whole body into such a position as he could rest in, and also able to resist any attempt to move him, and to talk, to a certain extent, rationally, his rapid death seemed very unusual. On the following day a *post mortem* examination was made. The body was then entirely livid and distended with gas. It was green in colour and the muscles of the spine were entirely decomposed. This rapid decomposition was due to the combination of two things—namely, first, the nature of the nerve injury, which in some way affected nutrition with great rapidity; and, secondly, the fact that he was saturated with alcohol. When we shifted the soft tissue out of the gutters at each side of the spinous processes, the first point that was evident was that we were dealing not with an ordinary fracture of the spine, but a dislocation. On the right side the articular process of the sixth cervical vertebra was exposed almost completely, but still it was at the upper part protected or concealed from view for a short distance by the process of the vertebra above it. The fifth articular process on the right side has slipped upwards, so as to expose nearly the whole of the opposing articular facet. On the opposite side the dislocation is complete. The entire articular process of the sixth cervical vertebra is exposed, and the process above it escaped completely into the notch in front of it. Just a little grain of bone, not much bigger than a hempseed, has been chipped off from the upper process, as it slipped over the lower. Beyond that there is no fracture. In front there is a good deal of distortion, while the fifth vertebra is advanced. There is a twist of the body strongly over to the right side. The left side, on which there is the more complete dislocation, is rotated forwards considerably, so that the left side of the spine is advanced in front of the right. The common ligament is twisted most markedly, and stripped from the lower vertebra, but still retains a slight connexion with it. Behind, all the ligaments, interspinous and ligamenta subflava, are ruptured. There is no fracture whatever of the bodies. The fibrous inter-vertebral disc is broken, but no bone is detached from the vertebral bodies. With respect to death in the cases recorded in our Transactions, we find that it occurred in Dr. Adams' case on the twelfth day, in Dr. Hutton's on the fourth, and in Dr. Little's on the sixty-eighth. The present case was more rapid in its issue than any of them. In the first two cases there is no statement of the exact nature of the dislocation—that is, as to whether it was complete on both sides or incomplete. Dr. Little's case was an example of complete luxation of both articular processes—this differs from it in being complete on one side only. In order to preserve the specimen, we have not exposed

the cord behind. I have no doubt that the cord has been ruptured to a great extent, and compressed, but that a great part of it remains. The heart and abdominal viscera were examined, but no cause of death was found in them. He fell backwards on his head, and had a scalp wound over the occipital protuberance. In all recorded instances of this dislocation the form of the violence was a forced flexure of the spine. I have no doubt whatever that the cause of death was extremely rapid decomposition, from the causes I have already mentioned. Whether there was any special vasomotor nerve lesion or not I am unable to say. The cause of the decubitus was perfectly apparent. There was a unilateral displacement of the spine, and a great amount of twisting, and the most advanced part of the displaced articular process presses directly on the nerve arising between the fifth and sixth cervical vertebræ; and it is obvious that the man lay in the position which was alone able to bear off that pressure.—*December 7, 1878.*

Malignant Disease of the Axilla; Secondary Disease of Liver and Spleen.—**SURGEON-MAJOR JACKSON** said: The following specimen of cancer was taken from the body of a man who had been in the army. It was lately dissected in our hospital. The details were as follows:—

W. M., 7th Dragoon Guards, age, thirty-three; service, ten years; admitted to hospital at Royal Infirmary, Phoenix-park, on July 22, 1878. Surgeon J. W. Martin, under whose charge he was, reports that there was a large nodular tumour occupying lower part of right axilla. W. M. stated it began on the march with his regiment to Dublin, and assigned as a cause the pressure of the strap of his water bottle. When first noticed it was the size of a pigeon's egg; no pain, but somewhat tender on pressure. It increased in size after he came to Dublin, and interfered with the performance of his duty.

On admission the tumour was hard, oval-shaped, and irregular on surface; size, $5\frac{1}{4}$ by 4 inches, apparently formed by the induration of several glands; a network of veins can be seen in the skin covering it; the tumour is fixed. Pain exists along the arm and at the shoulder; some numbness on the radial side of right wrist and thumb. Iodide of potassium and biniodide of mercury were applied locally, and iodide of potassium externally, and an anodyne liniment locally to relieve pain. Treatment did not cause any improvement.

Some decrease was found in the measurement of tumour on 1st August. Surgeon Joynt assumed charge of the patient on 2nd August. Patient complained of neuralgic pains in head; and on 10th that there was a sensation of numbness over the scapula posteriorly, and also over back and external side of right arm.

August 18th. Pains extended from mastoid process up side of head. The tumour in axilla gradually increased in size.

23rd. Was seen by Professor Bennett, who, having made a careful examination of the case, advised against any operation.

September 2nd. Pain in head persistent, in spite of blistering and other remedial measures; to have chloral and bromide of potassium at bedtime; suffered also much from pain in right groin, hip, and down leg to outside of knee; this was relieved by hypodermic injections. His general health steadily decreased, and in November the feet and ankles became swollen and œdematous; could scarcely move; the right hand and wrist soon became swollen also.

December 1st. Bed-sores were found on hips; extreme weakness; œdema extending up thighs.

Died on December 10th.

Post mortem examination of W. M., 7th Dragoon Guards, forty-eight hours after death:—

Body.—Upper portion extremely emaciated; lower, œdematous; bed-sore over left trochanter.

Head.—Scalp adherent to the skull over both parietal bones; brain healthy.

Thorax.—In right axilla a hard irregular-shaped tumour, measuring 4 by 4½ inches, not adherent to skin, situated under the greater pectoral muscle, firmly adherent to the lesser pectoral muscle, and to a part of the serratus magnus.

Lungs.—Upper portion of right lung was soft and of a reddish colour, but did not contain tubercle or any cancerous infiltration; left lung healthy; heart 9 oz., healthy; aorta and pulmonary artery healthy.

Abdomen.—Spleen enlarged, weight 15 oz.; surface blue; nodulated and hard; on section, was seen to be occupied by rounded cancerous masses, leaving scarcely any portion of the original tissue of the organ.

Liver.—The right base, at its upper and outer part, contained a rounded cancerous mass, the size of a small orange; in left kidney a cavity the size of a pigeon's egg—this contained grumous fluid.

No enlargement of glands of mesentery, nor was any abnormal state of stomach or intestines seen.

For the history of above I am indebted to Surgeons Martin and Joynt, the latter of whom made the *post mortem*, and furnished me with a report of same. As I never previously had seen cancerous deposit in the spleen, I thought the case worthy of being submitted to the Pathological Society. In the "Transactions of Pathological Society of London," I could find but two cases. In Vol. XX., p. 369, is one in which small granular growths were found in the spleen when cancer existed in other organs; and in Vol. XXIV., p. 223, in which a tumour was found in spleen, and was supposed to be unique as an example of primary cancer of spleen, no other new growth being found in the body. In "Lebert's *Traité d'Anatomie Pathologique*," Plate 128, 2, in a case where cancer of

stomach, liver, spleen, and portal vein existed, new growths, somewhat like those in the specimen now shown, were found.

In Vol. II. of his "Manual of Pathological Anatomy," Rokitansky remarks, "Cancer occurs very rarely in the spleen," p. 177. We have as yet only met with the medullary variety in combination with cancer of other organs, especially of the liver and lumbar glands. The structure of the spleen appears to afford a satisfactory explanation of the fact that cancer occurring in it is frequently invested by a fibrous sheath, within which it passes into a state of ichorous solution, &c.—*December 21, 1878.*

Changes resulting from Syphilitic Disease of the Larynx, and from Tracheotomy.—DR. BENNETT said: For the opportunity of presenting the present specimen I am indebted to Surgeon-Major Carte. The patient was a military pensioner. When I first saw him, about four years ago, he was about thirty-one or thirty-two years of age. He had been invalided, I do not know from what particular cause beyond general ill-health. He had served abroad for a short time, but as to the details of his service I have little information. On the morning of his admission into Sir Patrick Dun's Hospital, now nearly four years ago, he was labouring under respiratory distress so great that the question of immediate tracheotomy was entertained. We were able to recognise the cause of his suffering almost at a glance. He was in such distress that he could give no account of himself. He had no prominent syphilitic symptoms or eruption, but on examining his throat we found that the entire of the soft palate and all the fauces were cicatrised by by-gone syphilitic ulceration. His breathing was for a few hours relieved by treatment; but about three or four o'clock in the afternoon of the day of his admission, it was found necessary to open the trachea. The form of operation I adopted was the laryngo-tracheotomy of Boyer. It was performed without difficulty, and once the tube was in its place the distress was altogether removed, and the man completely relieved. He remained in hospital two or three weeks, breathing by the tube, and then left, wearing it, and we lost sight of him.

Last summer I was asked by Dr. Carte to see him again. He had been admitted into the hospital at Kilmainham, and got so strong and seemed so free from any active disease or sign of syphilitic ulceration, that the idea occurred to Dr. Carte that he might be allowed to give up wearing the tube. This was three years or more after the operation. I made an examination with the laryngoscope, but found no trace of active disease, or anything except cicatrisation and deformity. I agreed in the suggestion that he should try to go without the tube. He then got out on some term of leave, and drank freely, the result of which was that he was very nearly asphyxiated. He himself restored the tube to its place in the trachea—a point of some importance, because it usually happens that if a

tube be removed for even a few hours the wound, no matter how old or recent, contracts so much that it becomes extremely difficult to replace it. But he had no difficulty in replacing the tube, and again his trouble ceased. He continued his drinking as far as he could, and from that time his health broke down. About a fortnight or three weeks ago he was attacked with sudden pleuritic effusion in the chest, which displaced his heart towards the right. He passed into an extremely low condition and died a few days ago.

At the request of Dr. Carte I made a *post mortem* examination, the results of which are before the Society. Commencing from without we have the integuments at the site of the operation. The opening is about three-eighths of an inch wide, and a little longer in the vertical direction, slightly oval, having a perfectly even margin, and without any trace of ulceration, but having a sort of false mucous membrane or lip round it. It opens directly below the border of the thyroid cartilage; near the opening on the skin beneath it there is a small amount of ulceration, which was kept up apparently by the accumulation of mucus rather than anything else. The tissues, both deep and superficial, were otherwise perfectly healthy. The dark lymphatic glands towards the bifurcation of the trachea are quite normal. On opening the mucous membrane of the pharynx, I found a patch of great thickening and induration, amounting almost to cartilage. This was what we saw through the mouth during life. The epiglottis is deformed in a marked manner, and the edges of it eroded; there is no ulceration, but everything was cicatrised, and there was a slight change of colour resulting from fibrous degeneration. When I press the glottis together we get the condition presented during life, for when the examination with the laryngoscope was made it was easy to see that there was great deformity of the glottis, the arytenoid folds being displaced so almost to overlap one another. That condition is remarkably well preserved. All the region round the glottis is very vascular, but there is no ulceration. Passing down the œsophagus there is nothing to note except a considerable amount of deep congestion of the lower portion of it, which, I fancy, was principally brought about by gastric irritation that existed for some time before death. On making a dissection of the œsophagus off from the trachea, and exposing the trachea behind, I found that one or two of the small lymphatic glands contained caseous matter, and there was also a minute abscess in one gland. Some irritation from the wound, probably, gave rise to this condition. On dissecting the œsophagus entirely up, and opening the trachea, the first thing that strikes me is the remarkable contraction of the trachea as we approach the seat of the operation. Elsewhere normal, it there contracts so that the point of my little finger fills it. Its size is that of the tube last worn. Dissecting upwards we find that the muscular ridge of the cricoid cartilage is extremely developed, and its muscles extremely developed, while

there is a very strong keel or projection in the middle line. There is complete ossification of the posterior aspect of the cricoid cartilage—a point worth noting, considering that the man was only thirty-five years of age. No other part of the laryngeal structure, except the lower part of the thyroid, has undergone ossification, and the cartilages of the ribs were entirely free from degeneration. No doubt the excessive ossification of the cricoid was produced by the irritation of the disease, as well as of the tube, and the artificial defect resulting from the operation. There is no ulceration of the interior of the larynx, but the same condition of cicatrisation as in the pharynx. The normal yellow of the vocal cords is gone, and both are of a dull gray colour. The parts below the opening are perfectly free from any disease, except a few scars of healed ulcers. In the lower lip of the artificial opening there is a small fistula easily seen on the mucous membrane, very minute externally, capable of admitting a small-sized probe. It is not easy to explain the existence of this fistula, except that during the operation a slight wound was inflicted by the hook used to fix the trachea. Above the opening also there is a little depression on the mucous surface, from which there leads a narrow passage between the mucous membrane and the thyroid; along this a probe passes to the anterior attachment of the vocal cords, where it touches a small portion of uncovered cartilage. No doubt this upper sinus is the remains of an abscess. There existed almost no trace of syphilitic marking over the surface, but on the front of the tibia there were a couple of small nodes not prominent, but quite characteristic. The liver is of the fatty form described as nutmeg, with patches here and there over it, which are, I think, to be regarded rather as the cicatrised remains of syphilitic inflammation than as the mark of ordinary cirrhosis. It should be noted that the patient spoke well, though with a hoarse voice, while he stopped the tube with his finger.—*December 21, 1878.*

INSUFFLATION OF BISULPHATE OF QUININE IN GRANULAR CONJUNCTIVITIS.

M. RUVIOLI records two observations of severe pannus consequent on granular conjunctivitis, which were cured by this means when the habitual remedies had been tried without effect. However, bisulphate of quinine would be of no use when the pannus was accompanied by erethism of the ciliary nerves or when it is of strumous origin.—*Annali di Ottamologia and Montpellier Médical.*

S. W.

TRANSACTIONS OF THE CORK MEDICO-CHIRURGICAL SOCIETY.

SESSION 1878-9.

President—DR. D. D. DONOVAN.

Secretary—DR. D. C. O'CONNOR, Jun.

On the Treatment of Post Partum Hæmorrhage by the Injection of Hot Water into the Uterus. By WILLIAM HUBERT HOLMES, one of the Physicians to Cork Lying-in Hospital.

MR. PRESIDENT AND GENTLEMEN,—As far as I am aware, we are indebted to Dr. Atthill, Master of the Rotunda Lying-in Hospital, for first bringing the subject of the treatment of *post partum* hæmorrhage by the injection of hot water into the uterus, under the notice of the profession in Ireland, in a paper read by him before the Dublin Obstetrical Society on December 8th, 1877, and published in the January number of THE DUBLIN JOURNAL OF MEDICAL SCIENCE, 1878. In the paper referred to, Dr. Atthill cites two cases in which he tried the treatment with markedly good effect. The first case occurred on Nov. 20th, 1877, and the patient was rescued from a condition of great danger. The second occurred on Nov. 30th, and although complicated by the presence of a large fibrous tumour, the effect was instantaneous and permanent. With regard to these cases Dr. Atthill makes the following observations:—

“The cases I now record undoubtedly establish this much—that the injection of hot water powerfully stimulates the uterus to contract, and thus rapidly checks the hæmorrhage; but that it does more is, I think, as clearly established; it evidently acts as a general stimulant. The effect on the pulse was most marked—indeed the pulse was affected more rapidly than by the hypodermic injection of ether, and it did not flag again.”

Before proceeding to the relation of my own cases, a few words more as to the origin of the treatment. It would appear that in the year 1874 Dr. Whitwell, of San Francisco, saw the uterus contract firmly and instantaneously upon being washed out with hot water after an operation by Dr. M. Sims upon a sarcomatous growth of the fundus uteri. Dr. Whitwell tried the plan afterwards (August, 1875) in *post partum* hæmorrhage, with perfect success, and having communicated with Dr. Foley, of Boston, who, some time previous to November, 1877, was studying in

the Rotunda Hospital, the circumstances came under Dr. Atthill's notice, and induced him to give the method a trial, with the excellent results already mentioned.

Dr. Whitwell^a sums up the advantages of the hot water treatment as follows:—

“1st. It is easily obtainable.

“2nd. It is absolutely safe, if care be taken to exclude air from the tube.

“3rd. It stops hæmorrhage, not by artificial plugging, but by causing a natural contraction of the womb.

“4th. It is cleanly, and a disinfectant, such as carbolic acid, can easily be added.

“5th. By imparting heat it rallies an exhausted patient, and gives power to the muscles for contracting, instead of—as in the case with ice—abstracting what little heat remains, and so benumbing and paralyzing.”

I now proceed to the relation of the cases which came under my own notice:—

CASE I.—On the 15th of February, 1878, I was hastily summoned to attend a Mrs. J., who lived about two miles out of town, and who was stated to be in a dying condition. On my arrival I ascertained that she had been delivered of her sixth child about two hours previously, and that before delivery some considerable hæmorrhage had taken place. The placenta was expelled naturally—the child was dead.

The patient was quite cold, insensible to outward impressions, and I could scarcely feel the flicker of a pulse at the wrist. The bed and bed-clothes were saturated with blood, a pool of blood lay on the floor, and a thin stream slowly trickled from the uterus. On applying the hand over the uterus, that organ felt large and spongy. I had Higginson's syringe with me, and the solution of perchloride of iron, which I had found effective and safe on former occasions. However, having read Dr. Atthill's paper in the January number of *THE DUBLIN MEDICAL JOURNAL*, I determined to give the hot water plan a trial. Accordingly, having removed a quantity of clots from the uterus, and introduced the tube nearly to the fundus, I slowly injected hot water to the amount of about three pints, until it returned nearly colourless. The uterus contracted firmly, showing no disposition to relax. All bleeding ceased at once, the pulse became fuller and stronger, the patient showed signs of returning consciousness, and presently expressed herself as feeling “so comfortable.” She made a good recovery, if we except a moderate attack of phlegmasia dolens—a not infrequent sequence of severe hæmorrhage—and I had the satisfaction of seeing her long after in robust health.

CASE II.—On May 20th I was called to a wretched, half-starved

^a Medical Record.

dispensary patient, aged forty, placed under the most unfavourable hygienic conditions, in labour with her first child, and, owing to pelvic disproportion, it was evident that delivery with the long forceps was necessary. Having consulted with my friend, Dr. Donovan, I extracted a living child after some difficulty. Immediately after extraction an alarming gush of blood took place. I removed the placenta, applied firm pressure over the fundus, and gave ergot freely, notwithstanding which the uterus remained spongy, and bleeding still continued—the woman, who could not afford loss of blood to any extent, becoming blanched and cold. Encouraged by the previous case, I now injected hot water until it returned nearly clear. The uterus contracted well in a few minutes, all bleeding ceased, and the patient made a good recovery.

CASE III.—On November 3rd, 1878, I attended Mrs. B. in her first confinement, about three miles from town. The second stage was protracted; head large; pains feeble and inefficient. I delivered with the long forceps without much difficulty; the child was alive, but with an immensely elongated head. Smart bleeding ensued; ergot was given; but after waiting some time there was no uterine action sufficient to expel the placenta, which I accordingly removed. I got my assistant to press the fundus firmly, and gave ergot freely, but profuse bleeding still took place. I now determined to inject the hot water, and was not disappointed in the result—the uterus contracted firmly, all bleeding ceased, and the patient recovered without a bad symptom.

In these three cases, more especially the first two recorded, I have not the slightest doubt that the patients were in the process of dying before my eyes, and I have as little doubt that their lives were saved by the hot water injection; and although I should not wish cases of *post partum* hæmorrhage to occur for the purpose of testing the method, yet, when occasion arises, I should be glad if any gentleman present would give the hot water injection a trial.

SALICYLIC ACID ERUPTION.

AMONG other unpleasant consequences of the use of salicylic acid in some constitutions, is an eruption of an urticarial character. Dr. F. Freudenberg describes a case in the *Berliner med. Wochenschrift* (No. 42, 1878), where the whole back of the patient was covered with ecchymotic patches, which extended to the sides and chest. The acid was discontinued, and the eruption slowly disappeared, and was quite gone by the sixth day. The acid was then resumed, and again the eruption broke out, so that the medicine had to be definitively renounced. The patient was anæmic, which may explain the hæmorrhagic appearance of the spots.—*Med. and Surg. Rep.*, Jan. 4, 1879.

COMMENTARIES ON DISEASES OF THE KIDNEYS.*

PART II.

By ARTHUR WYNNE FOOT, M.D., Univ. Dubl.; Senior Physician, Meath Hospital and County of Dublin Infirmary; Fellow and Ex-censor, King and Queen's College of Physicians in Ireland; Diplomate in State Medicine, Trinity College, Dublin; Lecturer on Practice of Medicine in the Ledwich School of Medicine and Surgery; Fellow, Royal Geological Society of Ireland, &c.

[Continued from page 187, Vol. LXVII.]

RENAL CALCULI AND RENAL COLIC. Description of renal colic. Indications of one kidney only being affected. Anuria in renal colic. Treatment of nephritic colic—means of relieving the pain—Nature of the concretions which excite renal colic. Diagnosis of nephrolithiasis (stone in the kidney) comprehends that of the variety of stone and integrity or otherwise of each kidney. Urinary sediments different from urinary sand or gravel—how to discriminate. Results of irritation of nephritic calculi—pyelitis (calculous) and pyelo-nephritis. Hæmorrhage a symptom of renal calculi—renal colic from transit of blood-clots. Duration of nephrolithiasis—Terminations—Prognosis. Pathological changes produced in the kidney, its pelvis, and the ureter, by renal concretions. Hydronephrosis. Chemical composition of renal calculi. Relation to gout. Influence of age and sex. Etiology of renal calculus—usual nucleus.

Renal Calculi and Renal Colic.—A kidney may contain one or more calculi, varying in size from that of a pin's head or hempseed to that of a horse-bean. A concretion permanently lodged in the pelvis of the kidney may attain a weight of several drachms or ounces, and acquire a branched or arborescent form, grotesquely like the antlers of a stag or a piece of coral. Renal calculi are sometimes wholly latent. They may even attain a large size, and destroy extensive portions of the kidney without betraying their presence by a single symptom. It also not infrequently happens that small urinary calculi, which certainly must have been formed in the kidney, are voided with the urine, the patient never having suffered the slightest inconvenience prior to their emission. This silent discharge of renal calculi is, however, exceptional, as usually their transit from the kidney is attended with a peculiar series of symptoms to which the term renal colic has been applied. Sometimes

* The text of the essays of Professor Carl Bartels, of Kiel, and Professor Wilhelm Ebstein, of Goettingen, in Volume XV. of the "Cyclopædia of the Practice of Medicine," edited by Dr. H. Von Ziemssen, has been followed as closely as possible, with additional notes from all the best authorities on the subject.—A. W. FOOT.

patients awake in the middle of the night from quiet sleep, racked by the most torturing pain that has seized them as quick as lightning. In other cases the attacks are induced by active or passive movements of the body, or exertions, such as riding, driving, running, jumping, or by labour of the most various kinds—even by such acts as sneezing, coughing, &c. The pain, which is of the most severe kind, extends from the region of the affected kidney towards the bladder; it is not confined to the region of the kidney and the course of the ureter; it spreads over the entire abdomen, or radiates to the breast, penetrating to the shoulder-blades, or runs along the false ribs or the crest of the ilium. It is often accompanied with pain in the labium or testicle of the affected side, or the pain may radiate to the extremity of the glans penis and the inside of the thigh. Every movement is torture to the patient, as it increases his pain. During the attacks people bend themselves up double, or endeavour to find relief from the pain by lying on the painful side, with their thighs drawn up. The countenance is pale and bedewed with cold sweat, the pulse is small, and the hands and feet cool. In spite of an incessant desire to micturate, the urine is scanty, often mixed with blood, and voided in drops, with burning pain. The testicle on the affected side is painful, and is drawn up against the abdominal ring. Prout^a met with several cases in which pain and swelling of the testicle constituted one of the first and most prominent symptoms produced by a renal concretion. The pain is accompanied by a great mental trepidation; it is so overwhelming that it may paralyse the most powerful man—he becomes as helpless as a child, and trembles as if in mortal terror, while heavy sweat drops stand upon his forehead. In irritable subjects there may be general convulsions, which are to be regarded as reflex phenomena. It is not unusual for pregnant women to abort during attacks of renal colic. Troja reports an observation of a case in which a woman with calculous disease aborted fourteen times. Vomiting and retching almost always accompany this frightful pain; the manifestations of gastric sympathy sometimes play such a prominent part as to create a doubt about the diagnosis. In the passage of a renal calculus a large share of the pain is referred to the bladder, and a painful spasm of the bladder often arises with severe strangury or ischuria. These attacks may pass off in a few hours, and rarely last over twenty-four hours. Relief comes, often quite suddenly. The patient may feel something drop into the bladder, and, all at once, his agony is past. In spite of the formidable aspect of its symptoms, life is hardly ever in immediate danger in renal colic. Sometimes the concretion fails to clear the ureter and becomes impacted in some part of its course, which is about eighteen inches in length. In such a case the subsidence of the symptoms is more gradual and less complete, and a temporary remission is soon followed by renewed and

^a On Stomach and Renal Diseases. 5th ed. P. 318.

violent torment. When a calculus becomes firmly wedged in the ureter, the severe attacks of renal colic are due, not so much to the irritation of the mucous membrane by the concretion, as to the distension which the urinary passages experience from the urine accumulated behind the obstruction. The colic is called forth by the futile peristaltic movements which take place from time to time in the abnormally distended urinary canals. In the majority of instances concretions that have once entered the ureter pass through it, and when the obstacles to their passage have been successfully overcome the attack of colic is at an end. In one set of cases this occurs at the end of a few hours, in others these attacks last for days, and even sometimes for weeks. It is well known that the ureter is narrowest at its lowest portion where it opens obliquely into the bladder, and this is the place at which concretions are usually retained the longest; and here, too, by causing the most complete retention of urine, they produce the worst symptoms. Gigon, however, in a recent work upon the ureter (*Union Méd.* 1856) has shown that the middle portion of this canal, which used to be considered cylindrical, presents a constant fusiform dilatation; it can then easily be imagined how a calculus lodged in the narrow upper portion, which Gigon calls the neck of the ureter (*col urétrique*), can find itself at liberty in the median fusiform dilatation of the canal, and how the symptoms remit without the calculus having reached the bladder. It can also subsequently provoke a fresh attack of colic when lodged in the most narrow termination of the ureter.* Sometimes, during an attack of renal colic, instead of the usual scanty and bloody urine, an entirely colourless urine, as clear as water, is secreted. This occurs when only one kidney is affected with calculus, and its ureter is obstructed, while the other kidney is healthy, performs its functions normally, and its ureter is unobstructed. If both kidneys have calculi, or if the individual has but one kidney, and this is so affected, then there may be complete cessation of the escape of urine, accompanied by all the conditions belonging thereto, provided that both ureters are occluded (or the single ureter, where but one exists), and the escape of urine is entirely prevented. If the obstruction to the escape of urine is not removed in time, death will follow, usually with convulsions, and coma, generally, at the latest, ten days after the beginning of the attack. In some instances, however, people seem capable of recovering, even after a long-continued anuria. For example, in a widow, sixty-three years of age, who had suffered fifteen years with symptoms of stone, Salgado witnessed recovery following the escape of a calculus the size of a bean, with much gravel and an abundance of urine, *after complete anuria had lasted for thirteen days.*

The treatment of nephritic or renal colic, caused by the passage of a calculus along the ureter, is quite a different thing from the treatment

* Woillez. *Diagnostic Médical.* P. 212.

of the conditions which lead to such formations, and the severity and urgency of the symptoms in many cases call for much prompt decision on the part of the medical attendant, and may be such as to tax his resources to their utmost.

The pain is so acute that opium must be administered in full doses and at short intervals. If syncope threatens from the sense of deadly faintness, which in some patients accompanies the pain, it will be judicious to administer some fluid preparation of opium in brandy and water as hot as it can be swallowed. When the irritability of the stomach prevents the exhibition of remedies by the mouth, a suppository in the rectum or a hypodermic injection of morphia will be advantageously employed, and the combination of morphia and atropia is specially suitable in the convenient form of the discs made by Savory and Moore, each containing $\frac{1}{8}$ grain morphia and $\frac{1}{120}$ grain atropia. Niemeyer observes that the anæsthetic action of the opium is insufficient to account for the relief which usually follows this treatment. It is much more likely that the muscular fibres of the ureter, which have been urged into a state of contraction by the irritation of the stone, which they have spasmodically grasped, are relaxed by the narcotism induced by the opium. Inhalation of chloroform vapour seems to have an action similar to that of opium, and is perhaps the next best remedy. Belladonna is sometimes useful when opium fails or disagrees, and when a calculus is descending may be of special service in relaxing the spasmodic grip of the ureter which embraces the calculus and impedes its progress. A hot bath, if it can be conveniently had—either a hip bath or a full length one—frequently gives the greatest ease in the passage of renal, as well as of biliary calculi. Of its utility in a case of the latter kind there is striking evidence in the narrative of the passage of a gall stone by a medical man,^a who, when the pain had amounted to torture, forcing him to scream loudly and vainly seek relief in every position, on getting into a warm bath, experienced instantaneous relief, and so complete that he fell asleep while in it. Large hot linseed-meal poultices in relays, or India-rubber bags filled with water as hot as can be borne, applied to the back and loins, are comforting and grateful to most patients. It will be necessary owing to their extreme restlessness, and their constantly shifting their positions in search for one of ease, to secure the poultices in contact with the skin. Prout observes that he has occasionally known the greatest relief from the intolerable burning sensation of which patients sometimes complain with calculi, composed of calcium oxalate or phosphates, to be obtained by the application of pounded ice to the region of the kidney. He first took the hint from a patient who always applied it in his own case with the best effect. He sagely remarks that the practice should be limited to the forms of calculi mentioned, and not be

^a Dub. Quart. Jour. Med. Vol. XXI. P. 436.

had recourse to in plethoric, gouty individuals, labouring under lithic acid calculi. Copious enemata of warm water, with or without opium, act beneficially—first, by taking pressure off the ureters, by unloading of the large intestine, and afterwards by internally fomenting the irritated parts. Should the stomach be tolerant of them, the patient may be encouraged to take warm drinks of an unstimulating nature copiously, so as to produce a free secretion from the kidney, in order that, if possible, the urine pressing after the stone may help to drive it before it into the bladder. Of such drinks weak tea is one of the most useful and agreeable, or warm water and gin for patients of the male sex. Should a state of anuria be induced by the impaction of the calculus, it is well to bear in mind that Sir James Simpson relieved this formidable accident in two cases by a measure as simple as it was ingenious. He caused the patients to be held head downwards, and at the same time rubbed the affected side. The concretions changed their position and fell back into the distended renal pelvis, and the discharge of the urine was again established.

As a matter of course these attacks of renal colic are only observed when concretions attempt to pass the ureters which are too large for them. If the concretions are small enough to pass the ureters without obstruction, as in the case of renal sand, or of the smaller forms of gravel, or if they are so large that they cannot leave the renal pelvis, then, of course, renal colic cannot occur. The greater the obstacle to the escape of urine presented by the stone, the more intense will be the attacks of colic. If the urine can pass down into the bladder alongside of the calculus, then the backset and the colic do not attain a high grade. In a certain series of cases the attacks of colic do not end suddenly, but slowly and gradually. There are cases in which stones of but slight consistency crumble into smaller particles of gravel, or into sand, within the ureter. The longer calculous disease of the kidney lasts, and the more frequently the attacks recur, the milder the renal colic often grows, though this is unfortunately by no means always true. This depends on the fact that the accumulation of fluid accompanying each attack results in enlarging the calibre of the ureter. In some instances the attacks of pain recur only after months, or even years, and then do not reach a high grade, and have no injurious influence on the general health.

An efficient diagnosis of nephrolithiasis, as the condition of stone in the kidney is technically termed—aside from determining whether renal calculi exist at all—should arrive at some conclusion as to the variety of concretion present, and as to whether one or both kidneys are involved. To accomplish this object we have to attend on the one hand to the examination of the urine, and on the other to the symptoms presented by the case. The examination of the urine is very important, because it often gives us evidence of stone in the kidney at a time the most propitious for therapeutical interference. The urine often contains gravel

consisting of the ingredients of the urine which compose the concretions. The diagnosis is rendered certain if actual little concretions appear in the urine. Especial importance is to be attached to those sediments which appear immediately after the evacuation of the urine, which, therefore, exist as such in the urine, as opposed to those which are thrown down only after the urine cools. Aside from these powdery amorphous deposits consisting of urates, larger grains of gravel are often seen, varying from the size of a poppy-seed to that of a pin-head, which often escape observation in the midst of the amorphous sediments. They can be got hold of most easily by shaking up the sediment found at the bottom of the vessel; then the larger grains, being the heavier, fall to the bottom again first. In patients with renal calculus, even when the urine contains no sandy or gravelly sediment, it is not rare to find an abundant deposit, consisting principally of the epithelium of the renal *pelves and calyces*.

If the condition is complicated with calculous pyelitis, mucus, pus, blood, and sometimes the characteristic cells from the pelvis of the kidney, will be found mingled with the urine in variable quantity. The greater the irritation caused in the renal pelvis and in the kidney by concretions, which is especially great in case of the rough, thorny, and very hard calcium oxalate stones, the higher will be the grades of pyelitis and pyelo-nephritis developed, and the more abundant will be the purulent admixture in the urine, together with which renal sand or gravel, or small fragments of stone, may be found. For a long time it was a matter of dispute whether one kidney was more liable to nephrolithiasis than the other, and, if so, which one. Morgagni was one of the first who agitated this question, and decided in favour of the greater liability of the left kidney to this disease. If the renal colic is on the right side, it is not always certain that we are actually dealing with a case of renal stone, for cases are not wanting—although they are extremely rare—in which (a communication existing between the gall-bladder and the ureter) biliary calculi have found their way into the urinary bladder. In one of these cases nine gall-stones, and in another two hundred, were passed with the urine during a week's time. Generally, indeed, the occurrence of highly icteric urine without icterus of the body would be sufficient to lead to such a diagnosis.

In the diagnosis of calculus in the kidney great importance has been attached—and that not without reason—to periodically-recurring renal hæmorrhages; for the most frequent cause of renal hæmorrhage is the change of position of a calculus developed within the kidney, and the wounding of the tissues caused thereby. It is, however, to be remembered that certain cases of renal calculus run their course without any hæmaturia, and, furthermore, that the hæmaturia itself may introduce a source of error, since the passage through the ureter of fibrinous clots,

caused by the hæmorrhage, may give rise to attacks which are completely analogous to those of nephrolithiasis. In the hæmorrhages which accompany renal cancer the transit of blood-clots may give rise to symptoms quite analogous to those of calculous renal colic. Especial care is necessary in judging of those cases of nephrolithiasis in which the patients complain only of bladder symptoms; but, if the urine is bloody and purulent, with pain in the loins—especially if this is localised in one side—the question of whether there is not a stone in the kidney must arise, even though the individual may refer all his symptoms to the bladder. In an original affection of the bladder the symptoms are all referable to this organ long before pain in the loins appears. The diagnosis of whether but one kidney is affected, and, if so, which one, independently of operative procedures, has a prognostic significance. Calculous disease generally attacks but one kidney, and the other kidney vicariously performs the function of the diseased organ. The fact of the colic pains occurring only on one side justifies the conclusion that the kidney of that side is diseased, but not that the other kidney is entirely healthy. There is one symptom which argues with the greatest probability in favour of the healthy condition of one kidney—viz., when, during the attack of renal colic—in which the ureter of the diseased kidney is so obstructed that none of its secretion can enter the bladder—perfectly normal urine is passed instead of the abnormal secretion which has been customary. Relying especially on this symptom, Simon extirpated a calculous kidney. The autopsy afterwards showed that the other kidney was sound. Another evidence in favour of the calculous disease being unilateral, which is not to be under-estimated, lies in the fact of the calculus being composed of phosphates, for this fact justifies the conclusion that the development of calculus is due to a purely local cause, whereas the development of uric acid concretions often has its foundation in constitutional causes which are by no means limited to one kidney.

Duration, terminations, prognosis.—Nephrolithiasis is ordinarily a very chronic affection. It is very rare for any acute process to induce a sudden termination; but this may occur when, as the result of the arrest of a renal stone during its passage through the ureter, a rupture of the latter takes place, whereupon sudden peritonitis quickly destroys life; or when, in consequence of the complete retention of urine by obstruction of its channel of escape, uræmia is developed, which usually causes death within a few days, with coma and convulsions. Those cases in which the renal parenchyma suffers from the pathological results of nephrolithiasis generally assume a very chronic course, because the other kidney usually assumes vicarious functions. Nephrolithiasis may terminate in (a) recovery or (b) death. The former issue obtains in those cases in which gravel or small concretions can pass the ureter, and, having reached the bladder, are expelled with the urine, when the formation of

such sand or gravel comes to a stop, either spontaneously or as the result of appropriate treatment. Even in the case of the larger renal concretions where destruction of the kidney has taken place, recovery has been observed, owing to a shrinking of the diseased kidney, after which the wasted organ causes no further disturbance, and, if the other kidney is healthy, or provides for the secretion of urine, life may continue undisturbed. A fatal termination may be brought about, as we have just said, by acute processes, but it is more frequently effected through a series of chronic pathological changes, which result from the presence of concretions in the renal organs or their appendages, and which slowly but steadily undermine the general health.

The *prognosis* depends, among other things, largely upon early and judicious treatment, if such be cheerfully submitted to by the patient, and upon the amount and degree of disturbance of the renal organs which the nephrolithiasis may have set in action. The earlier the disease comes under treatment, the better, in general, is the prognosis. In a certain series of cases it is possible to effect a diminution in the development of new sand and gravel which might give occasion to the formation of larger concretions. However, so-called cures are frequently only temporary improvements, and after a pause of years the former troubles may return. If renal stones have existed for a long time, and if they have caused disturbances in the renal tissue, then the prognosis is much more unfavourable.

It is now time to inquire into the *pathological changes produced in the kidney, the pelvis of the kidney, and the ureter, by renal concretions*. In nephrolithiasis one kidney generally remains healthy, but calculi are often developed in both sides. In such cases the stones in the two kidneys are sometimes of different chemical composition. The alterations caused by renal stones differ according to the number, size, situation, and composition of the concretions. In the lighter grades of irritation of the pelvis by calculi, the principal disturbance consists of a more or less highly developed pyelitis. The entire mucous membrane, which is otherwise pale, is found injected. Sometimes this injection is very great, the membrane assuming a deep red appearance, which, in more advanced cases, is accompanied by numberless little ecchymoses. There may be more or less extensive participation of the renal tissue, so that in some instances the most extreme pyelo-nephritis follows. When a stone within the pelvis of the kidney becomes so large that it cannot pass the ureter, it still continues to grow, at first filling the renal pelvis partly, afterwards entirely, and sometimes the calyces too. The urine secreted above the stone is dammed up in proportion to the amount of hindrance offered to its escape. The pent-up urine exercises considerable pressure upon the renal tissue, in consequence of which an ever-increasing atrophy of the renal parenchyma, from pressure, is developed; so that, finally, nothing

of the entire kidney is left but a membranous sac, enclosing a number of cavities that communicate with one another, and represent the former highly dilated calyces of the kidney. This condition—dilatation of the pelvis of the kidney, with consecutive disappearance of the renal tissue—is called *Hydronephrosis*, a term first used by Rayer. When a purulent pyelitis passes over to the tissue of the kidney itself, causing pyelonephritis, ever-increasing portions of the renal tissue then gradually break down under purulent degeneration, and finally nothing is left of the kidney but a sac filled with pus and stones, which often attains a larger size than the normal kidney. This sac is formed of the capsule, very much thickened, and usually is extensively adherent to neighbouring organs. Concretions which are impacted in the ureters may there cause inflammation and ulceration of the walls of these tubes, with perforation of the same, whereupon, as a general rule, death very quickly follows, with symptoms of peritonitis. J. P. Frank tells of a nun, in Cremona, in whom the left ureter, which was grown fast to the wall of the abdomen, was eaten through by a stone in such a manner that after the latter had given rise to an abscess it worked its way outward through the abdominal muscles.

The chemical composition of renal calculi is various. Those consisting of uric acid are the most frequently met with. The distinguished Swedish chemist, Scheele, who discovered uric acid in 1776, believed that all renal concretions were composed thereof. The same relationship exists between uric acid and renal calculi as between cholesterine and gall-stones. Both exist but sparingly in their respective secretions, but contribute prominently to the formation of concretions on account of their slight solubility. About five-sixths of all renal stones are more or less uric acid formations. They may occur at any age, but are especially frequent in persons who have passed middle age, and more particularly in gouty subjects. The old writers laid great stress upon the contemporaneous occurrence of gout and renal stones. Erasmus wrote to a friend: "I have renal disease, and thou hast the gout—we have married two sisters." Renal calculi have a peculiar interest in relation to stone in the bladder, as it is well known that the majority of urinary concretions are developed in the kidneys, and that vesical calculi are formed by the enlargement of concretions that have descended from the kidney. According to Heller, the proportion of urinary calculi which originate in the kidney, as compared with the bladder, is as 100 to 1. Renal stones occur with notable frequency during the period of childhood—a fact which contrasts remarkably with what is observed in reference to gall-stones. The male sex, in the case of adults as well as of children, is far more liable to urinary calculi than the female. Against 5,497 cases occurring in males, Oesterlen finds only 309 in females.

With reference to the etiology or causation of renal stone, there is

very little accurate information. This much may be depended on, that the amorphous powdery sediments which are evacuated with the urine constitute the starting-point and basis of what are ordinarily called renal gravel and renal stones, but the specific etiological cause is, as yet, unknown, which admits of innumerable cases of the abundant and continued formation of gravel without the formation of a stone. With regard to the solid bodies, which are liable to serve as a nucleus for crystalline formations, the greatest probability is in favour of mucus produced within the pelvis of the kidney, while it is also possible that tube-casts and minute blood-clots may form the starting-points of renal concretions.

[To be continued.]

WHY PHYSICIANS SHOULD USE THE METRIC SYSTEM.

DR. EDWARD WIGGLESWORTH gives the following reasons in a recent circular :—Because this system most nearly approximates to a perfect one, embodying, as it does, the most careful and delicate work of the International Metric Commission, composed of scientific men from all countries. Because it is *international* ; and medicine is as cosmopolitan as human nature itself. Because of its great *convenience* in writing and compounding prescriptions, in dividing doses and in computing quantities required during given times. Because of its *safety*, due to its *uniformity* and *simplicity*. It may be learned in five minutes. In complexity there is always danger, and the resemblance of the signs of the scruple, drachm, and ounce has more than once proved fatal to human life. The metric system dispenses with the signs of the quantities, employs Arabic figures instead of Roman numerals, and assures the physician of more competent service, because from more educated pharmacists, such being always the first to adopt it. It is decimal, and a perpendicular line instead of the decimal points obviates any possibility of error from this source. It is allied to the change already made by Americans, from pounds, shillings, and pence, to dollars and cents. Because of its *delicacy* and *accuracy* for the chemist and the pharmacist ; and here the beauty of the system is especially apparent, for it provides denominations of weights applicable to the smallest quantity which the physician can prescribe, the old grain being by far too large and coarse a unit for modern medicine. Moreover, the English and American graduates are both in use in this country, and yet are not alike. There is a difference of eighteen grains in the weights of their fluid ounces. Then, too, if *f* is omitted before the ounce symbol, either the graduate or the troy ounce may be used. Finally, because it deals preferably with weights alone, *while admitting the use, if desired, of both weights and measures, as at present.*—*Medical and Surgical Reporter.*

SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P.

VITAL STATISTICS

Of Eight Large Towns in Ireland, for Four Weeks ending Saturday, January 25, 1879.

Towns	Population in 1871	Births Registered	Deaths Registered	DEATHS FROM ZYMOTIC DISEASES							Annual Rate of Mortality per 1,000 Inhabitants
				Small-pox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhoea	
Dublin, -	314,666	752	1127	88	24	25	2	8	43	17	46.6
Belfast, -	182,082	581	582	—	1	22	2	14	21	7	41.5
Cork, -	91,965	217	270	—	—	—	1	7	7	2	38.3
Limerick, -	44,209	80	151	—	—	—	—	12	—	4	44.5
Derry, -	30,884	89	54	—	6	—	—	—	—	1	22.8
Waterford, -	30,626	64	54	—	—	2	—	—	4	—	22.8
Galway, -	19,692	27	57	13	—	—	—	—	—	1	37.5
Sligo, -	17,285	83	49	—	—	—	—	2	—	1	36.8

Remarks.

Except in Derry and Waterford, where the death-rate was moderate for the time of year, the returns of mortality were very unfavourable. In the metropolis particularly the death-rate was excessive—the deaths registered being 1,127, or 375 more than the births registered (752). In London the mortality was at the rate of 27.2 per 1,000 of the population annually; in Edinburgh it was 22.9; and in Glasgow, 28.2 per 1,000. Within the municipal boundary of Dublin it was 49.2, and in the Dublin registration district at large, 45.5—even when the deaths of persons admitted into public institutions from without the district are deducted. The deaths from zymotics were 229 in Dublin, being 84 in excess of the average number in the first four weeks of the previous ten years (145.4). Small-pox, measles, scarlatina, and fever, all show a considerably increased fatality, and there is good reason to believe that several of the deaths returned as due to “diarrhoea” (17) and “convulsions” (68) were really caused by measles, scarlatina, or fever. At the close of the four weeks the number of cases of small-pox under treatment in

the Dublin Hospitals was 188, compared with 122, 86, and 47, at the close of the three preceding periods respectively. Of the deaths from fever, 8 were ascribed to typhus, no fewer than 30 to typhoid, and 5 to continued fever of undetermined type. In Belfast, scarlatina, whooping-cough, and fever, were again very fatal. In Cork the epidemic of whooping-cough continues. Small-pox was not so fatal in Galway. Respiratory affections caused 878 deaths in Dublin, including 297 from bronchitis and 48 from pneumonia. They were, however, more destructive to life in the first than in the second fortnight. The previous ten years' averages were—respiratory diseases in general, 193·8; bronchitis, 149·5; and pneumonia, 23·0 deaths. The increase in the mortality from zymotic affections is the most disquieting feature in the Dublin returns for the period.

METEOROLOGY.

Abstract of Observations made at Dublin, Lat. 53° 20' N., Long. 6° 15' W., for the Month of January, 1879.

Mean Height of Barometer,	-	-	-	30·008 inches.
Maximal Height of Barometer (on 27th at 9 p.m.),	-	-	-	30·564 „
Minimal Height of Barometer (on 14th at 9 p.m.),	-	-	-	29·461 „
Mean Dry-bulb Temperature,	-	-	-	35·0°
Mean Wet-bulb Temperature,	-	-	-	33·1°
Mean Dew-point Temperature,	-	-	-	29·8°
Mean Elastic Force (Tension) of Aqueous Vapour,	-	-	-	·168 inch.
Mean Humidity,	-	-	-	81·5 per cent.
Highest Temperature in Shade (on 13th),	-	-	-	49·4°
Lowest Temperature in Shade (on 12th),	-	-	-	21·2°
Lowest Temperature on Grass (Radiation) (on 12th),	-	-	-	18·3°
Mean Amount of Cloud,	-	-	-	70·8 per cent.
Rainfall (on 10 days),	-	-	-	1·714 inches.
General Direction of Wind,	-	-	-	S.E. and E.

Remarks.

The month was a very wintry one. Although the mean temperature was 2·5° above that of December, 1878, yet it was 6·6° below the average temperature of January in the previous thirteen years (41·6°), and exactly equal to that of the exceptionally cold January of 1867. The week ending Saturday, the 11th, was perhaps one of the most inclement of the present unusually severe winter. Piercingly cold S.E. gales blew almost without intermission from the 6th to the 10th inclusive, and there were keen frosts on the 5th and 11th. During the stormy period just spoken of, cirrus cloud was constantly travelling in an upper westerly current. Accompanying the S.E. gale of the 7th, much sleet, hail, and snow fell. On the 10th the maximal temperature was 31·6°, although

the wind was blowing a gale off the sea. In the period from the 12th to the 18th the weather was comparatively mild—notwithstanding, the thermometer fell every night below 32° on the grass. Indeed there were only two nights in the whole month when the exposed thermometer did not fall to 32° . On the 12th a rise of temperature amounting to 27.0° (from 21.2° to 48.2°) took place, with a S. wind, and at this time the highest and lowest readings of the shade thermometer were recorded within 36 hours of each other. From the 19th (on which day 2.84 inches of rain fell at Valencia, in Kerry, owing to the intermingling of a cold with a warm current of air), temperature gave way gradually, an anti-cyclone over Scandinavia throwing a cold, dry, easterly current over the British Islands. Severe, and almost continuous, frost prevailed in central England, but in Dublin it was not so cold, because the prevalent winds were S.E. or off the sea. On the 25th, 26th, and 27th, however, it froze hard—the sky being comparatively clear and the air dry and nearly calm. During the last three days the anticyclone became re-established over Scandinavia, and the S.E. wind again set in over the British Islands, where overcast skies and cold bleak weather were generally experienced. Snow or sleet fell on 8 days; hail also on 8 days. Fog was observed on the 5th, 26th, 27th, and 28th. Solar halos were seen on the 5th and 9th, lunar halos on the 1st and 3rd.

It is noteworthy that the cold weather in November and December depended on a distribution of atmospherical pressure the very reverse of that which determined the equally cold weather of January. During the former period the barometer was *low* over Scandinavia, and consequently the prevailing winds in the United Kingdom were *north-westerly*; during the latter period the barometer was generally *high* over Scandinavia, the result being a great preponderance of dry and keen *south-easterly* winds in the United Kingdom.

ADULTERATION OF SANTONIN.

ALARMING results have occasionally been noticed to follow the administration of medicinal doses of santonin. A sample of apparently pure santonin—offered, however, at a remarkably low quotation, and bearing the name of a foreign manufacturer—was lately found by Mr. Stevenson (*Pharm. Jour.*, Nov. 16, 1878) to contain 61.8 per cent. of crystallised boracic acid. [This is, we believe, not a very uncommon adulterant of santonin. Its presence may be recognised by an alcoholic solution of the sample burning with a green flame.—ED., “PERISCOPE.”]

PERISCOPE.

Edited by G. F. DUFFEY, M.D., F.K.Q.C.P.

THERAPEUTIC RESULTS WITH PILOCARPIN.

THE results of recent investigations are here summed up. Dr. Demme, of Berlin (*London Medical Record*), arrives at the following conclusions:—

1. Pilocarpin is an effective diaphoretic and sialagogue in childhood.
2. It is borne very well, in appropriate doses, even by children of very tender years.
3. Unfavourable after-symptoms are but rarely observed, and, probably, may be altogether prevented by the administration of small doses of brandy before the injection.
4. The conditions in which it is chiefly indicated are the parenchymatous inflammations of the kidney with dropsy, following scarlatina.
5. Pilocarpin does not appear to exercise an influence on the heart's action.

The Hospital Gazette states that an important physiological effect of pilocarpin, according to Dr. Zielewicz, of Posen, is its power to reduce animal heat. He has observed a decrease of temperature amounting to as much as 2, $2\frac{1}{2}$, and even 3 degrees, averaging, however, 1 to $1\frac{1}{2}$ degrees. In very few instances there was a slight increase of the temperature. Again, it seems doubtful to me whether the diminution of the temperature can be attributed primarily to the action of pilocarpin, or whether it is not due to, and only temporarily caused by the evaporation of the perspiration. Zielewicz arrives at the following conclusions:—

1. Pilocarpin is a reliable diaphoretic in the diseases of children.
2. The unpleasant symptoms which occasionally follow the administration of this remedy interfere with its more general use.
3. To eliminate or diminish these complications the following rules should be observed:—
 - a. The dose of pilocarpin should, be as small as possible.
 - b. A small amount of morphia should be administered with the pilocarpin, best in the proportion of ten parts of hydrochlorate of pilocarpin to one part of hydrochlorate of morphia.
 - c. To prevent collapse a few drops of camphorated oil should be added to the solution.

Dr. Felsenreich, assistant to Professor Gustav. Braun at the Vienna General Hospital, observes that Dr. Massman's statements (*Medical Times and Gazette*, July 13th, p. 56) on the employment of pilocarpin in the induction of premature labour must lead to further inquiry into the action of this substance on the uterus. At Professor Brann's request he tried its efficacy in nine cases of ataxy of the uterus, with reference to its future employment in cases of hæmorrhage produced by this cause. In but three of the cases did the hypodermic injection take effect, and that only at the end of ten minutes; so that it cannot be

regarded as a suitable means for combating active hæmorrhage, in which promptitude of action is so important a factor. As in these cases, too, there is no time to examine the action of the heart, another contra-indication arises, for, as Petrina has shown, whenever this action is in anywise abnormal, the greatest care is required in the administration of pilocarpin, for arrhythmia or an arrest of its action may then be easily induced. Indeed, as any considerable hæmorrhage does greatly disturb the action of this organ, this itself is a contra-indication. These considerations do not apply to the induction of premature labour, and additional trials of the power of pilocarpin for this purpose may be made without danger. Dr. P. K. Kretschmar adds, in *The Hospital Gazette*, that the hydrochlorate of pilocarpin, derived from the alkaloid found by E. Hardy in the leaves and in the root of *pilocarpus pinnatus*, is in many respects the most valuable of the preparations of *jaborandi*. It comes in small, white crystals, very soluble in water, and is for different reasons especially adapted for *hypodermic* medication. Its action resembles that of the drug itself, but it is *more uniform* and reliable than either the infusion or the fluid extract. It also influences the bronchial secretions by making them more fluid, and it has been used with advantage in croup, bronchitis, &c. A solution is made by dissolving $\frac{1}{2}$ a grain of hydrochlorate of pilocarpin in 30 minims of pure water. Dr. Kretschmar uses, in cases of children from six to ten years of age, 10 minims of this solution, $\frac{1}{8}$ gr. hypodermically, and repeats the injection once or twice the next or following day. To adults he has given 20 minims ($\frac{1}{3}$ gr.), repeated every day for three days. The simplicity and almost painless manner of its administration, the fact that its hypodermic use does not cause any irritation or abscess at the point of injection, the easy manner by which it can be administered in a state of uræmia, unconsciousness during convulsions, &c., makes it a most valuable remedy in the treatment of children. Dr. Kretschmar used it in five cases of parenchymatous nephritis following scarlet fever, four of which occurred in children under twelve years of age, and states that its action was very satisfactory, although it produced considerable vomiting in one and moderate emesis in another case.—*Medical and Surgical Reporter*.

NERVE-STRETCHING IN TETANUS.

DR. MUDD reports a case of nerve-stretching in tetanus which was followed by no good results. "It was the case of a man whose hand had been crushed and who had a gangrenous ulcer on the back of it. He had tetanus, but I am not sure how long he had it before I saw him. The dorsal and palmar surfaces were so much involved that the only local treatment we could use was emollient applications. The man's condition was unsatisfactory—temperature high, pulse rapid, and he was suffering intensely. The spasms were irregular—not severe—but well marked,

with great difficulty of breathing and deglutition. We adopted the suggestion of nerve-stretching. We cut down upon the musculo-spiral, ulnar, and median nerves, at the border of the coraco-brachialis muscle, near the point where the musculo-spiral passes behind the humerus, and stretched each of the branches. To determine the amount of stretching, I measured the amount of force applied. I lifted the nerves by the handle of the knife and caught one branch between my finger and thumb of one hand, and hanging a weight to the little finger of the other hand, grasped the nerve with thumb and finger of this hand, relaxed the muscles, and let the tension of the weight fall upon the nerves. I think I used four pounds on one nerve, and possibly five or six pounds on the others. Only one nerve seemed to yield. Am unable to say what amount of sensation or motion remained; but the spasms continued, and I believe became more violent after the operation. The patient died the next day."—*St. Louis Med. Jour.* In the *Centralblatt f. Chir.*, Professor Vogt has published the case of a man in whom tetanus supervened on the fifteenth day after his hand was wounded. There was marked episthotonos, and the patient seemed likely to succumb, when, on finding that pressure on the brachial plexus produced pain and convulsions, it was determined to lay bare the nerve-trunks and stretch them. This was accordingly done in the subclavicular triangle. The neurilemma seemed congested. On waking from the chloroform the patient was able to put out his tongue. Several partial spasms occurred, but the tetanus had vanished. The only abnormal feeling that remained was occasional formication in the fingers.

S. W.

SALICYLIC ACID OINTMENT IN ECZEMA.

If Fleischmann may be believed, salicylic acid will be found superior to every other topical application employed at present in cases of eczema. Fleischmann uses an ointment composed of about half a drachm of the acid to an ounce of some fatty base [*e.g.*, vaselin], which is applied over the affected surfaces. After a certain amount of desquamation, the skin soon assumes its normal aspect.—*Rev. des Sci. Méd.*, Jan., 1879.

THE MOST EFFECTUAL METHOD OF CONTROLLING HIGH TEMPERATURE AFTER OVARIOTOMY.

DR. T. G. THOMAS (*N. Y. Med. J.*), after some interesting remarks on the history and value of clinical thermometry, figures and describes Dr. G. W. Kibbee's "fever cot." "The bed on which the patient lies consists of a strong, elastic cotton netting, manufactured for the purpose, through which water passes readily to the bottom below, which is of rubber cloth, so adjusted as to convey it to a vessel at the foot." A folded blanket is placed on the cot to protect the patient's body from the netting, and at one end is placed a pillow covered with India-rubber

cloth, and a folded sheet is laid across the middle of the cot and about two-thirds of its extent. The patient is laid upon this, her clothing is lifted to her arm-pits, and the body enveloped in the folded sheet, which extends from the axillæ to a little below the trochanters. The legs are covered with flannel drawers and the feet with warm woollen stockings, and bottles of warm water are placed against the soles of the latter. Two blankets are placed over her, and the application of water is made. Turning the blankets down below the pelvis, the physician now takes a large pitcher of water (75° to 80°), and pours it gently over the sheet. It saturates this and percolates the network, is caught by the rubber apron beneath, whence it is discharged into the receiving vessel. The patient now lies in a thoroughly soaked sheet, with warm bottles to her feet, and is carefully covered up with dry blankets. The water is only applied to the trunk. The affusion lasts from ten to fifteen minutes, and the water collected in the tub at the foot of the bed, after having passed over the body, is usually from 8° to 12° warmer than when poured from the pitcher. The result of the affusion is tested by the thermometer at the end of every hour; if the temperature has not fallen, another affusion is practised until the temperature falls to 100° or less. The patient lies constantly in a cold wet sheet, which never becomes a fomentation; because as soon as it abstracts sufficient heat from the body to do so, it is again wet with cold water. He has kept patients two or three weeks on this cot, enveloped in the wet sheet without discomfort, and with marked control over the animal heat. Dr. Thomas does not propose by it to check peritonitis, or cut short septicæmia, but to rob these diseases of hyperpyrexia, to resist the primary assault in the hope of bearing up against a more prolonged but less violent siege.—*St. Louis Med. Journal*.
S. W.

FLOATING SPLEEN.

DR. DIVARIS, of Taganrog, describes (*Jour. de Méd.*, Juin, 1878) what he believes to be an example of the lesion. The patient was a woman, aged thirty-four, the mother of seven children. In the interval between the birth of her sixth child and her last pregnancy she had complete prolapse of the womb. During the two years subsequent to her last delivery she suffered from intermittent fever, with relapses from time to time. The history of the case, the general feebleness and relaxation of the tissues, the presence of a smooth, elastic, freely-movable tumour in the abdomen corresponding in shape to the spleen, the complete absence of dulness in the splenic region, the fact that the tumour could be placed, with a feeling of relief to the patient, in the position normally occupied by the spleen, and, finally, the negative signs of any other tumour, led Dr. Divaris to believe that the tumour was the spleen itself. He suggests that increased weight of the organ may have been the cause of its

displacement. [Several cases of dislocation of the spleen are referred to by Dr. Wardell, in his article on Diseases of the Spleen, in the fifth volume of Reynolds' "System of Medicine."—ED.]

UNGUENTUM VASELINI PLUMBICUM.

PROFESSOR KAPOSI, of Vienna (*Wiener mediz. Wochenschrift*, No. 17, 1878), proposes a new use for vaselin—its substitution for olive or linseed oil in the manufacture of the diachylon ointment of Hebra. It is well known that a decided objection to this most valuable remedy exists in the penetrating, disagreeable odour, in consequence of the decomposition of the oil in the process of heating with the lead. Vaseline is not decomposed under these conditions, and the ointment prepared from it is odourless, and remains unchanged in this respect when applied to the skin. It has the same consistency as the ordinary salve, and possesses no irritating properties when applied to the inflamed skin. It is made by heating together equal parts of empl. diachyl. simplex and vaselin. It may be perfumed with a trace of oil of bergamot or lavender, or with Peruvian balsam. Vaseline itself, Kaposi finds, possesses no active properties whatever upon diseased skin, acting merely as an emollient.—*Boston Med. and Surg. Journ.*

TREATMENT OF CHYLURIA.

DR. RYAN has had an opportunity of treating this disease in the person of a native lad. The urine had a specific gravity of 1020, it was of a light cream colour, and deposited a thin coating of red blood-corpuscles on the sides and bottom of the containing vessel. It gave an abundant precipitate amounting to nearly $\frac{1}{10}$ ths of its volume on heating, and with nitric acid; it became somewhat opaque on the addition of liquor potassæ, and cleared up completely on adding excess of sulphuric ether. Fat globules and altered blood-corpuscles were visible under the microscope. There were renal casts. Beyond an occasional feeling of languor, the patient seemed to experience no symptoms of constitutional disturbance. Mist. creasoti, B. P. $\mathfrak{z}\text{i}$. ter in die., was ordered, the patient being recommended to abstain as much as possible from fat. The urine rapidly became free from opacity under this treatment, and no trace of albumen could be detected. The creasote was administered with a view to destroy the *filariæ sanguinis*, which presumably cause this disease, by giving rise to a communication between the lacteals of the intestine and the minute blood-vessels in their neighbourhood. Dr. Ryan states that no inconvenience attends the internal administration of creasote, and that it may be given in much larger doses than were employed in this case without producing any ill effects. A superficial microscopic examination of the blood failed to show the presence of *filariæ*.—(*The Indian Med. Gaz.*, Oct., 1878).—*The Practitioner*, Feb., 1879.

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PART I.

ORIGINAL COMMUNICATIONS.

ART. XII.—*Clinical Contributions to Diseases of the Spinal Cord.**
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AMYOTROPHIC LATERAL SCLEROSIS.

THE great advance made in the knowledge of diseases of the spinal cord, owing chiefly to the labours of Erb, Charcot, and Leyden, has had, amongst its other good effects, that of encouraging the practice of accurate observation of phenomena which previously were regarded merely as strange and inexplicable. Notwithstanding the obscurity which still shrouds many points in the histology and physiology of the nerve centres, sufficient progress has been made, and sufficient interest awakened, to enable us to make our records of cases of some value when the attempt is made to refer the various manifestations of nervous disease to more general laws than are yet known or understood. There need, therefore, be no hesitation in recording the notes of a case uncommon in its occurrence and in its nature:—

CASE.—The patient, a tall and powerfully-built man, aged thirty-four, was admitted into hospital under my care on the 2nd of October, 1878. He was a brewer's drayman, accustomed to lift heavy weights, and exposed to the influences of cold and wet weather. In the early part

* Read before the Dublin Biological Club, on Tuesday, March 11, 1879.

of last February the patient was in his usual health, being then a strong muscular man. In the latter part of the month he complained of some pain and soreness in the left upper arm. It became fatigued upon exertion, and he found some difficulty in raising it, owing to its weakness and rigidity. Within a short time the forearm got weak, and a month afterwards a precisely similar condition was developed in the right upper extremity. He had a tired feel in both limbs; there were constant twitchings of the muscles, "as if something were alive in his arms;" he suffered occasionally from violent jerking movements in them, and they became rapidly wasted. In the following April he noticed some weakness in his left leg when walking, and shortly afterwards one morning, when getting out of bed, he fell to the ground. The right leg soon became affected, and a feeling of numbness was developed in both limbs; he felt his legs "sleepy," and he had sensations of creeping and tingling in them. An intermittent tremor attacked both limbs, often to such a degree as to arrest progression. It was generally developed during exercise, but it also occurred immediately after the patient got into bed and when he awoke in the morning. As the disease progressed the limbs became at times so stiff in extension that the patient was utterly powerless to move them. "They got like sticks," to use his own expression, and he often tried ineffectually to bend them with both his hands. The contracture often occurs when in bed, or when he stands, or walks for some time. Slight fibrillary twitchings were complained of in the gluteal muscles of both limbs.

At the same time that the lower limbs became affected, the patient noticed some difficulty in spitting, a dribbling of water from his mouth, which, when asleep, moistened his pillow, and a change in the expression of his face. Since then he has had some difficulty in swallowing. He suffered from attacks of giddiness in the head and occasional flashes of light before the eyes, which caused his sight to scatter. He felt occasionally his neck weak, so that if his head was thrown back he had a slight difficulty in bringing it forwards again. A few days before admission to hospital his voice became almost inaudible, but this lesion passed away in a few days. There was no history of any specific disease.

The points specially noticed during the time the patient was under observation in hospital are the following:—The face presents a somewhat stolid appearance; there is an absence of the movements of expression, at least as far as the lower part is concerned. The lower lip droops somewhat, and both lips are stiff in their motions. He can, however, blow out a candle and show his upper teeth, but he has lost the power of whistling, and he can spit out only by a great effort. The latter is effected by an explosive separation of the lips, and is accompanied by a sputtering noise. The movements of the tongue are *apparently* unaffected. The organ is, however, small, tremulous when protruded, and atrophied

towards the tip. The voice is decidedly nasal in tone; speech is slow, and gives rise, when continued for a time, to a sense of fatigue in the muscles employed. The lesion of articulation seems to be specially manifested in pronouncing the letters B, D, G, K, Q, R, S, U, Sch, W, and the words "brisk," "yes," "better," "mark," "think," and "key."

A curious point in connexion with the lesion of articulation was frequently observed. Ordinarily the pulse-rate, when the patient is at rest, is 72 beats in the minute; when he speaks for a time it rises to 96. This condition I have not seen noticed in any of the descriptions of bulbar palsy which I came across, though an abnormally rapid pulse, from paralysis of the vagi, has been recorded as occurring in the last stages of the disease.

The patient masticates his food more slowly than usual; occasionally a small particle gets attached to the back part of the roof of the mouth, and he is obliged to remove it with his finger, as the tongue does not press it away. His power of deglutition, according to himself, has improved, but he has still to make a number of efforts to get a bolus of moderately large size through the pharynx. The dysphagia seems to be due principally to weakness of the constrictor muscles. That the soft palate, however, is implicated is shown by the nasal character of the voice, and by the occasional regurgitation of fluids through the nose. The patient suffers from repeated attacks of sneezing—sometimes of so violent a character as to disturb his equilibrium, and he falls to the ground. The sneezing may, I think, be accounted for by taking into consideration the difficulty which the patient has in blowing his nose—owing partly to the loss of power in his hands and arms, and partly to the escape of the air through his separated lips when he attempts to do so.

Examining the condition of the upper extremities we found a very decided atrophy of the muscles of both limbs. There is extreme wasting of the thenar and hypothenar eminences, hollowing out of the interosseous spaces, atrophy of the muscles of the arm, the pectorals, the deltoids, and trapezei. Difficulty is experienced in executing any movement with either the hand or arm. The dynamometer registers a pressure of only 26 lbs. with the right hand, and 24 lbs. with the left. Sensibility to touch, heat, and pain, is, upon careful examination, found to be intact. Feebly-marked fibrillary twitchings are occasionally observed in some of the muscles, but there exists no evidence of muscular tension—in fact, there is scarcely any resistance to passive movements.

Dr. Walter Smith kindly made with me an electrical examination of the muscles. We found in the muscles of the forearm that the faradic excitability was greatly diminished, and in the palmar muscles it was almost lost. Galvanic excitability was also diminished, but not to the same extent as the faradic. Mechanical stimulation of the muscles seemed to be followed by the strongest contractions.

Passing to the state of the lower extremities, the first point noticed was the peculiar walk of the patient. The gait is a shuffling one. The patient sways himself forwards to one or other side, alternately dragging each leg, with the toes and outer side of the foot trailing the ground. The mode of progression is like that described as the spastic gait, but there is not that peculiar hopping elevation of the body dependent upon the reflex contraction of the muscles of the calf. Frequently, when walking, both limbs become rigid in extension, and for a time progression is arrested until the spasm has subsided. If the knee be slightly bent and the anterior part of the foot placed somewhat forcibly upon the ground, a tremulation of the entire limb ensues, which sometimes extends to the opposite one. This tremulation may be at once arrested by firmly pressing the heel upon the ground. It may be re-induced by abruptly flexing the foot upon the leg, the motion ceasing when the former is extended. The phenomenon appears in the most marked degree when the patient is in any way excited—as in examining him before the hospital class, &c. Rigidity of both legs may be produced by frequent passive movements of them, but it usually comes on spontaneously when the patient is after getting into bed, or attempts to leave it. During the state of rigidity the clonic spasm of the limbs cannot be produced. Great loss of motor power exists in both limbs; there is a sense of fatigue experienced in them upon exertion; the patient feels his legs very insecure when walking, but there is no evidence of muscular wasting to any marked degree. There are no signs of ataxy discernible.

Reflex sensibility to tickling is very much increased; the slightest touch to the sole of the foot causes the limbs to be jerked into different positions. This condition is, however, of variable intensity, being specially marked when the patient is warm in bed. There exists, moreover, a very decided increase in the reflex action of the tendons, shown by the effects following percussion of the ligamentum patellæ or the tendon of the quadriceps extensor. Of course the clonic tremulation of the leg points also to increase of tendinous reflex. I have not been able to develop it by percussing over the periosteum of any of the bones.

Beyond the paræsthetic phenomena of an occasional sense of formication and “sleepiness” of the legs, there is no lesion of sensibility to touch, heat, or painful impressions.

The faradic excitability of the lower limbs is not sensibly altered, but the passage of a strong current through them set up the clonic spasm of the muscles before alluded to. No very obvious change was observed with regard to galvanic stimulation. There is no affection, whatever, of the bladder or bowels—no tendency to constipation. The urine is normal in reaction and specific gravity. A short time before admission to hospital some failure of virile power manifested itself.

With reference to treatment I may briefly say that, during the com-

paratively short time the patient was under my observation, nitrate of silver, phosphorus, chloride of gold, and iodide of potassium, in large doses, were severally employed. The constant and induced currents were also tried. Some improvement seemed to result in the conditions dependent upon the bulbar lesion. In other respects the state of the patient was unchanged.

I may summarise, then, the prominent features of the disease as follows:—Weakness of the muscles of the arms, and their subsequent diffuse muscular atrophy; paraplegia, with prominent evidences of motor irritation and a spastic gait; absence of any lesion of the function of the bladder or rectum; increased reflex excitability of the cord with conservation of sensibility, and accession of bulbar palsy.

It is evident that the case belongs to the type of diseases of the spinal cord described by Charcot under the name of “*tabes dorsal spasmodique*,” and more recently by Erb as “*spasmodic spinal paralysis*.” The chief phenomena of the genus are:—A slowly-developed motor paraplegia; manifestations of motor irritation, evidenced by twitchings of the muscles and by extreme muscular tension and contractures; a peculiar, spastic gait, and a marked increase in the reflex action of the tendons. There are usually no atrophy of the muscles, no impairment of sensibility, and no lesion of function of the bladder, rectum, or generative system. It is in this affection that the *status* known as spinal epilepsy is markedly developed. During the seizure the lower limbs are rigidly extended—the thighs upon the pelvis, the legs upon the thighs, and the feet in the position presented in *talipes equino-varus*; the limbs seem to be almost cemented together, and can be separated from each other only by considerable effort. In such cases, as in the one just described, abrupt flexion of the foot upon the leg gives rise to a convulsive tremor not always limited to the limb experimented upon, but sometimes extending to the opposite one, and even occasionally causing tremulation of the entire body. This tremulation usually ceases spontaneously after a few minutes, but it may often be abruptly arrested by forcibly extending the foot upon the leg, flexing the great toe, or firmly grasping the front of the thigh. The convulsive seizure may also be produced by faradisation, pinching or kneading the skin, traction of the limb, as in taking off the boots or stockings, or by ordinary muscular exertions, as in getting in or out of bed, stamping the fore part of the foot upon the ground, &c. As is well known, the phenomenon of

trepidation has been made the subject of special memoirs by Westphal and Erb. It is generally admitted to be due to increased reflex tendinous action, starting in the foot from forcible stretching of the tendo Achillis.

An examination of negative and positive evidence warrants the attempt to localise the lesion in spasmodic spinal paralysis to the lateral columns of the spinal cord. Thus the absence of ataxia and of disordered sensibility, with the existence of reflex action of tendons, enables us to exclude disease of the posterior columns, of the greater part of the gray substance of the cord, and of the brain. The antero-lateral columns, the anterior gray horns, and some small portion of the gray substance, remain for the localisation of the lesion.

According to Ludwig and Woroschiloff the anterior columns of the cord may be looked upon merely as commissural connexions between the motor nerves and the anterior segments of gray matter, and the experiments of these observers go to show that the direct motor paths are in the lateral columns only. This view receives support from certain pathological observations. In the slowly developing contractures and increased tendinous reflex which follow disease of the corona radiata and internal capsule, round patches of sclerosis are found exclusively engaging the pyramidal tracts of the cord, especially that motor conduction-bundle found in the posterior half of the lateral column which is known as the crossed column of Flechsig.

In the descending degeneration which follows a transverse myelitis the same tracts are involved as those in lesions of the brain, and similar muscular phenomena are produced.

Again, in multiple sclerosis of the spinal cord, in which tremulation and contractures of the extremities with loss of motor power are such constant symptoms, lesions are invariably found in the lateral columns of the cord.

Thus it is seen that whenever tremulation, contractures, and motor paralysis exist, with other phenomena of disease of the cord, a lesion of structure is invariably found in the lateral columns. If there be evidence to connect these other phenomena with disease of other special parts of the nerve centre, have we not reasonable grounds for assuming that where signs of motor irritation and paralysis exist alone, the lesion in the cord is confined to its lateral columns?

I may now direct attention to a *species* of spasmodic spinal

paralysis, which was originally described by Charcot, and which, I think, deserves to be named after this distinguished observer. Under the name of amyotrophic lateral sclerosis, Charcot has described a form of symmetrical disease of the lateral columns of the cord in which the lesion extends to the large ganglion cells in the anterior gray horns, probably through the connecting commissural tracts. The history and signs of this affection are almost identical with that of the case recorded by me.

The disease begins in the upper extremities with loss of motor power followed by a rapidly-induced muscular atrophy of a diffuse character. Muscular tension and contractures of the upper extremities supervene. Some months afterwards the lower extremities are attacked in a somewhat similar manner—the muscular atrophy not occurring at the same early period, but the tremulation, muscular tension, contracture in the extended position, and increased action of tendons, being well pronounced. In the late stage, when the muscular atrophy in the lower extremities has progressed, the muscular twitchings and contractures subside. Finally, the degeneration in the cord extends to the nerve nuclei in the medulla oblongata, and bulbar palsy is developed, which hastens the downward progress of the disease. Charcot, Joffroy, and others, have recorded the *post mortem* appearances found in cases under their observation—viz., symmetrical sclerosis of the lateral columns, with degeneration and atrophy of the anterior gray horns and the large ganglion cells situated in them. The sclerosis appears first in the lateral columns of the part of the cord in relation with the upper extremities, extending after a time to the anterior gray horns. It has not yet been ascertained if a lesion of a *portion* of the gray substance can give rise to secondary degenerations, so that we do not know what relation the disease in the upper part of the cord may hold to that in the dorsal and lumbar segments, or to the condition of the medulla oblongata.

The condition of the reflex function of the cord in the case recorded is worthy of observation. In spasmodic spinal palsy increased reflex excitability has been generally noticed. Its existence may be accounted for in either of two ways—by assuming a lesion of the tracts which place the different parts of the cord in relation with the inhibitory centres, or by supposing a condition of irritation of the gray substance. It is certainly much easier comprehending the latter view, as we know nothing of the tracts concerned in inhibition. The increase of reflex function in my patient was

markedly variable in its activity within short intervals of time. I observed frequently that the reflex excitability of both skin and tendons was most marked when the patient was warm in bed. Of course the condition of the vasomotor system must largely influence the function of the cord, especially if, as it is believed, it possesses within itself centres of inhibition. Moreover, the inhibitory influences shown by physiologists to be transmitted from the periphery by sensitive nerves would serve in part to explain variation in the excitability of the cord. Thus I have often succeeded in arresting the tremor of the limb by slapping with considerable force the front of the thigh. This arrest would be accounted for by the supposition of Goltz, that the susceptibility of the centres which receive impressions for the reflex function is lessened when they receive other impressions at the same time; or it may be that the stronger impression diverts the nerve-force to some channel other than the reflex centres of the cord, producing for a time exhaustion of their activity. We have apparently instances of this in the arrest of such reflex acts as sneezing or hiccough by strong impressions acting upon the sensorium or emotional centres.

On the distinctive diagnosis of this case from progressive muscular atrophy, and from chronic poliomyelitis anterior, it is scarcely necessary to dwell. The loss of motor power preceding the muscular atrophy, the convulsive seizures and contractures of the lower limbs with absence in them of any marked evidences of muscular waste, sufficiently differentiate it from the former.

In chronic poliomyelitis anterior the paralysis commences in the lower limbs, the atrophied muscles are soft and flaccid, and there is complete loss of reflex action in the affected parts. These features at once distinguish it from amyotrophic lateral sclerosis.

Although in recording cases of any special disease, one may not add anything material to what has been taught by original observers as to its nature and signs, still there is some advantage in adducing evidences of conditions well and faithfully portrayed.

ART. XIII.—*Case of Subacute Spinal Paralysis in the Adult.* By M. J. MALONE, M.D., F.R.C.S.; Physician to Barrington's Hospital, and to the Lying-in Hospital, Limerick; Examiner in Arts, Royal College of Surgeons, &c.

THE infrequency with which cases of spinal paralysis (anterior poliomyelitis) in the adult have been observed and recorded encourages me to offer for publication a short account of one such recently under my care. As the patient lived several miles outside this city (Limerick) my visits were necessarily few and at irregular intervals, and I regret that my report of the case must be imperfect in proportion; nevertheless I trust it may be of sufficient interest to warrant its appearance in these pages.

CASE.—In the autumn of last year I was requested to see Mrs. —, a lady about twenty-four years of age, married a few years, and a mother. She was reported to be very ill, and I went to her without delay. I found her affected with a curious combination of symptoms. She seemed to suffer from an attack of gastro-duodenitis, had constant vomiting, and appeared deeply jaundiced, even by candlelight. She complained of tenderness in the region of the liver, and that organ was, in my opinion, a little enlarged. The stethoscope revealed a slight tricuspid murmur; the pulse was frequent; the temperature was nearly normal. But the principal feature of the case was a marked paresis of the four extremities, amounting to almost complete paralysis in the lower limbs. These latter were much wasted—conspicuously so in the sural regions. There was pronounced hyperæsthesia of the tactile nerves; subjective painful sensations were occasionally experienced, but there were no “jumpings” of the limbs. The patient was barely able to produce slight motion of her feet by her concentrated efforts. The upper extremities were affected in a considerably less degree, yet they, too, were very attenuated—objects could be but imperfectly grasped, and the tips of the fingers could not be satisfactorily approximated. There was drooping of the wrists, as in saturnine palsy. In a general way the extensor muscles seemed to be more affected than the opposing groups, and those farthest from the trunk decidedly suffered most. The patient's intellect was perfectly clear, the cephalic nerves were not in the least implicated, and the control over the sphincters was unimpaired. She lay helplessly on her back, and was quite unable to turn on either side without assistance. Considerable pressure over the spinous processes caused no pain, nor did the patient experience any sense of constriction round the body, though she thought that she had had such a feeling some time previously; but vague replies in the past tense to leading questions are not to be relied upon.

On inquiring into the history of the case, I learned that the paralysis had gradually progressed during the preceding six weeks from such slight symptoms as "tripping on the carpet," and a sense of fatigue after moderate exercise. As the weakness in the limbs grew more marked the patient was removed to the country for change of air. She affirmed that a few days previously she had been able to move about her room with the assistance of her attendants, but since that time she had rapidly fallen into the very helpless condition in which I found her. It was confessed that the patient had been addicted to long-continued habits of intemperance, and she had recently been subjected to a good deal of mental distress and worry.

From these symptoms and history the disease was diagnosed as subacute spinal paralysis, the anterior poliomyelitis of Erb, which had recently, and probably accidentally, become complicated with gastroduodenitis; and though the latter affection was much the less important in respect of final results, it demanded precedence in treatment—the stomach, in its present condition, being quite unable to retain ordinary food, not to speak of such medicines as I intended to employ for the spinal disease. In two or three days the gastric attack yielded to a carefully regulated diet, into which lime-water and milk entered largely. The liquor bismuthi (Schacht), with small doses of morphia, was found most serviceable in checking the vomiting; the mistura sennæ was used as an occasional aperient. When the stomach troubles had ceased the jaundice speedily disappeared.

The treatment of the principal affection was commenced by the administration of ergot and belladonna, with faradic currents to the muscles engaged. As the patient showed no sign of improvement the belladonna was soon omitted, and iodide of potassium given with the ergot, as recommended by Hammond. The doses of the drugs were speedily raised up to eighty grains of the iodide, and a fluid ounce of Long's preparation of ergot in the twenty-four hours. In the application of the electricity the moistened electrodes were placed directly on the muscles, or currents were sent along the principal nerve-trunks. The battery used was one of considerable power, and about an hour was devoted to each sitting. The patient took her medicine with great regularity, as she was most anxious to do everything to favour her recovery. She bore the large doses of the iodide of potassium very well; her diet was as nutritious as possible, and all stimulants were prohibited. Counter-irritation was employed over the region of the cervical enlargement. I had intended to blister the whole length of the spine, but the patient so besought me not—as she lay constantly on her back—that I was compelled to yield to her entreaties. She remained under treatment some two months, in which time I saw her about ten times. I used every effort to induce her to come into town, or to enter the pay wards of

Barrington's Hospital, where I could see her daily, and have her regularly faradised, but neither she nor her friends would consent to this desirable arrangement. While under observation she steadily grew worse. The electrical irritability of the muscles, impaired from the beginning in a marked degree, as is invariably the case in this disease, gradually disappeared, remaining longest in the lower extremities in the glutei. In the interval between one visit and another, the muscles of the arms, which had hitherto responded pretty well to the current, almost entirely lost their excitability. It was evident that the disease was advancing upwards, and gaining in intensity as it advanced; and my mental prognosis was that death would, in all probability, occur with bulbar symptoms. She died seven days after I last saw her; and though there were peculiar circumstances that prevented me from obtaining an accurate account of her state during the last few days of her life, I have but little doubt, from what I was able to glean, that the immediate cause of death was an extension of the disease to the medulla oblongata.

The etiology of this case of spinal paralysis is obscure. The patient had been undoubtedly intemperate, and free indulgence in beer is mentioned by Klose as a probable cause of the affection; but it is certainly difficult to understand the infrequency with which cases are seen in this country, judging from its literature, if the ingestion of alcohol should at all stand in the direct relation of a cause. The characteristic drooping of the patient's wrists suggested the possibility of lead-poisoning. At the time I was quite unaware that Remak and others had considered the central lesion in lead-poisoning as probably identical with that found in this disease. In the present instance there was an entire absence of evidence to favour the idea of plumbism, neither was there any history of marked exposure to cold, or of other circumstances commonly supposed to have a causal relation to the affection.

This was the first case of spinal paralysis in the adult that came under my observation, yet its diagnosis could present but little difficulty. The *negative* characters—absence of tenderness on pressure over the spinous processes, of tendency to bed sore, of sense of tightness round the body, of jerkings of the affected limbs, of reflex irritability, of rectal and vesical implication—pointed distinctly to poliomyelitis; and these were confirmed by the rapid atrophy of the affected muscles, and the early loss of response to the electric current. Whether the hepatic and gastric troubles were simply concurrent or not I cannot say, but the readiness with which they yielded to treatment makes me strongly of opinion that they were.

The prognosis was at first rather favourable. The results of the great majority of reported cases were calculated to make one hopeful of recovery, at least as far as life was concerned; but the steady extension of the paralysis upwards, and the perfect inefficiency of any remedies used to control it, soon made it pretty certain that the disease must end fatally. There was, unfortunately, not the least chance of a *post mortem* examination being permitted. No doubt the anterior gray horns of the cord would have been found the subjects of degenerative lesion, yet it is most desirable that previous observations should be strengthened in this particular.

This case differs from the bulk of those heretofore reported (occurring in the adult), in the early age of the patient, in the rapid extension of the disease, and in its fatal termination.

Amongst children spinal paralysis is much more common than amongst adults, and is generally known as "infantile" paralysis. I have seen three cases of it within the last three years. I cannot at all agree with Heine (as quoted in Ziemssen, Vol. XIII., p. 669), that it is invariably blooming, healthy children who are attacked. Two of my little patients (the three were girls) were wretchedly scrofulous, yet one made a splendid recovery—completely regaining power over the lower limbs, which for many months she had been quite unable to use—under the influence of a tri-weekly faradisation. She has since suffered from strumous disease of several bones. The second little girl I had for some time in the Children's Ward of Barrington's Hospital, but she was so very delicate and nervous that it was next to impossible to do anything for her, and she was soon removed by her friends. She had double talipes equinus from tonic contraction of the sural muscles. The third little girl was of the "healthy blooming" type. She was brought to me three months from the inception of the attack, which was ushered in with head symptoms, vomiting, &c., and occurred in the month of June. This was the only one of the three cases in which I could get anything approaching to an exact history. Here one lower limb alone was affected, and by the use of the induced current and of small doses of strychnia it seemed to be regaining its plumpness, and some of the muscles at least were beginning to respond to the will, when the little patient was rather suddenly removed from under my care, and I have not heard anything of her since.

Whether occurring in children or in adults, the disease seems to be essentially the same; but, while it is acute in children, it is

chronic, or at least subacute in the adult. The issue is generally in recovery, though it may be with permanent atrophy of some groups of the muscles, and paralysis and deformity as its natural consequences. It is to be distinguished from "progressive muscular atrophy" *mainly* by this character, that in poliomyelitis the paralysis occurs before the wasting of the muscle tissue; while in the other affection the atrophy first takes place, and the paralysis follows simply as its physiological result. The various symptoms already referred to enable the observer to satisfactorily differentiate it from other diseases of the cord.

The ablest and fullest accounts of spinal paralysis, as far as my knowledge of its bibliography extends, are to be found in Dr. W. A. Hammond's admirable treatise on the "Diseases of the Nervous System;" and in Dr. Erb's work on "Diseases of the Spinal Cord," constituting the thirteenth volume of Von Ziemssen's "Cyclopædia of the Practice of Medicine."

Those who are anxious to study its morbid anatomy, and the *rationale* of the treatment recommended (subjects which it does not come within the province of this paper to discuss), I would beg to refer to those authors.

ART. XIV.—*A Case of Tetanus treated by Chloral Hydrate.** By EDWARD PEELE, L.K.Q.C.P., L.R.C.S.I.; Physician to the Hospital for Incurables and to the South City Dispensary, Dublin.

THE following case presents several features of interest which lead me to place it on record. The youthful age of the patient, the unilateral continuance of the affection, and the steady improvement during the administration of chloral hydrate, are amongst the principal:—

CASE.—On the 6th of January, 1879, I was called to see a boy aged six years, whom I found lying on his back in bed, having a somewhat peculiar expression of face, and complaining of pain, which he referred to his stomach and left side. I was told he had a short cough, that his bowels had not been moved for some days, and on casually placing my hand on the abdomen considerable hardness was felt. Attributing the ailment to slight cold, with constipation—for I had examined the chest and found nothing abnormal—I deemed it unnecessary to put any further questions; prescribed an aperient, an expectorant-diaphoretic mixture,

* Read before the Dublin Biological Club, on Tuesday, March 18, 1879.

and desired the mother to let me know, if necessary, how the child progressed. She came soon, and as her account was not satisfactory, I again visited the boy. I now noticed that the patient had great difficulty in protruding the tongue, and that the peculiar expression of face was intensified. This aroused my suspicions; and as the hardness of the abdomen still continued, although the purgative had done its work efficiently, I asked if the boy had received any injury. I was told he had slightly hurt his foot. I then proceeded to examine him.

I found in the middle line of the sole of the right foot, corresponding to the head of the third metatarsal bone, the remains of a lacerated wound a little more than half an inch in length. This was nearly well, and required but little further treatment. The right leg was rigid and could not be flexed in the least, on exerting ordinary power; the left was also rigid, but could, without much difficulty, be partly flexed. The abdominal muscles were all very hard, the recti feeling like boards. The arms also were stiff, but not at all to the same degree as the legs. *Risus sardonius* was well marked—the jaw almost locked—he being able only to protrude the tip of the tongue. The muscles of the neck and back all very stiff—*opisthotonos* distinct. On approaching and handling him he became greatly distressed, but I never saw any of the violent spasms described by his mother.

On seeing the nature of the case I wished to have him removed to hospital. This the mother positively refused to permit. I did not relish the responsibility, but, as there was no help for it, I decided, in the first instance, to treat the case with chloral, which I at once ordered in six-grain doses four times a day. The effect was almost immediate—his mother stating that the attacks of spasm soon diminished, and he rested better.

From time to time I elicited the following account from the patient's mother:—A day or two before Christmas the boy wore, for the first time, a pair of boots, in one of which a peg of some sort was projecting. After a few days he complained that his foot was sore. On examining the foot—the right one—his mother found a “gathering,” about the size of sixpence, which she poulticed.

One or two days before she sent for me he complained of pain about the stomach and left side. These pains subsequently became so violent and distressing that she took him out of bed, put him on her knee, and rubbed him well with her hand, on the supposition that “wind was the matter.” He suffered intensely from spasm, during which “his stomach would rise up.” These attacks occurred at intervals of about half an hour, lasting for ten or fifteen minutes. Besides the hand-rubbing she poulticed and stuped him freely; he was not relieved, however, by the applications. Both night and day he had these spasmodic attacks, during which he would become “as stiff as if he was dead”—the jaw being

completely locked, and he appearing to rest on his heels and the back of his head. She noticed the peculiar expression of face, but thought it was caused by the pain in the stomach.

The boy improved so steadily after he was put on the chloral that I relinquished the idea of treating him with any of the other well-known remedies for this terrible disease.

After a few days his mother complained that he was refusing his medicine. I had noticed he was not going on quite so well, and this was the explanation. I now spoke to him, showing the necessity of strict obedience, and threatening to send him to hospital in case he again refused. Young as he is he fully understood me, and took whatever was given him afterwards with the greatest regularity and cheerfulness. In addition to the chloral, the treatment consisted in clearing out the bowels with an occasional dose of calomel, and giving nourishing liquid food.

Daily improvement continued, the spasms became less frequent, and the appetite, which had been bad, returned. My notes to the end of the case are as follows:—

Jan. 21st.—Can open the mouth pretty freely; still stiff in the jaw, neck, abdomen, and legs, but can flex voluntarily the latter, though not easily; arms, which had been affected, now quite mobile.

23rd.—Improvement continues; says his right leg is stiffer than his left; is lively; has no spasm or inconvenience on approaching or handling him. The expression of his face when I made him laugh was most amusing. The endeavour to laugh, mixed with the characteristic “risus,” gave him a most grotesque appearance. Opisthotonos now gone, but well-marked emprosthotonos has succeeded, his appearance reminding one of a stooping, round-shouldered old man. The dose of chloral increased to eight grains.

25th.—Still improving; right leg very stiff; got him up in bed to try if he could walk; can evidently use the left leg for locomotion, but the right he cannot use or control at all, and it appears not to belong to him. Of course he was supported in this attempt. After my visit he complained to his mother that his right leg was shorter than his left, and was greatly fretted thereat. She comforted him with the assurance that the doctor would make it all right.

28th.—Greatly improved; emprosthotonos nearly gone; muscles generally continue stiff and hard; “risus” still remains, though not so marked; can now better flex and extend the right leg in an effort to walk, which, with help, he managed to do fairly.

31st.—Makes a much better attempt at walking, very little assistance being now required; has a little cold; his mother states that he has great difficulty in sneezing; has still some “risus;” sits up in bed and amuses himself playing with his toys; has no attacks of spasm, but when defecating complains of great pain about the navel—doubtless owing to

the continued hardness of the abdominal muscles. Chloral continued, and, with it, quinine.

Feb. 3rd.—Can now raise himself to his feet without assistance; can also walk a few steps alone, but is fearful of falling. When standing he semiflexes the right leg. In pointing the right foot his action is as though he could neither flex nor extend it voluntarily; the toes are always flexed; the face still retains some of the characteristic grin. Cough troublesome to-day. Ordered the chloral in an ordinary cough mixture.

6th.—Is up and well; can walk without assistance, carrying the right leg stiffly; “risus” nearly gone; can cough and sneeze with greater ease.

13th.—With the exception of slight stiffness in the right leg, can walk well now; right foot straight; the flexed condition of the toes gone. He continues the medicine. The face still retains some of the peculiar expression.

March 17th.—He is now all right, and going about as though nothing had ever happened. I did not know him when I saw him this morning. Every particle of “risus” has disappeared, and the face seems quite a different thing. He has gained flesh. His eyes seem larger, and have altogether lost the peering look they had. Nothing could be more perfect than his recovery.

The symptoms in this case, so far as I can gather from closely questioning the mother, showed themselves from seven to nine days after the boots were first worn. Whether the foot was hurt on the first or some subsequent day I do not know. In any case the attack came on with moderate rapidity, and was not wanting in severity. From personal observation and the mother’s account I am disposed to think the diaphragm was first attacked—a circumstance which has been noticed as not unusual by many writers.

I have thought it worthy of being brought under notice for several reasons—amongst them,

1. The age of the patient.
2. The somewhat unilateral character of the affection.
3. The good effect of treatment by chloral hydrate.

1. It is admitted by eminent authorities that although tetanus occurs at all ages it is most rare under the age of ten, excluding, of course, the trismus of infants.

I am not aware of the exact ages of those referred to by Mr. Poland, but he states that, of 449 cases,

29	were under 10 years,
261	between 10 and 30 years,
122	“ 30 and 50 years,
37	over 50 years.

He also mentions that the youngest case on record of regular tetanus is twenty-two months, and the oldest seventy-five years.

The case I have brought under your notice is one occurring in a boy six years old, at which age the disease is not often met with.

2. The next point I wish to notice is the undoubted unilateral character of the disease in this case, at least so far as the injured limb is concerned—for only with regard to the right foot and leg can this be said, the rest of the body being equally affected on both sides. I have not as yet had an opportunity of looking up the literature of particular cases, but so far as general statements go the different authorities I have consulted do not, I think, mention any exaggeration of symptoms in the injured parts, with one exception, and that is a case given by Professor Nothnagel (German Clinical Lectures), which points decidedly to prominent symptoms on the injured side. His was a fatal case, and briefly is as follows:—A young and strong man, nineteen years of age, by trade a cartwright, injured the last phalanx of his left thumb with an axe. The wound bled freely, but was deemed so trivial that he continued his occupation. Seven days later, after sweating profusely at his work, he was exposed to a violent draught. Yet even after this there was not, as yet, the slightest pain in the thumb. Next day, symptoms of tetanus showed themselves, and he was removed to hospital. The patient did not complain spontaneously of any sensation or pains in the injured limb, nevertheless careful investigation discovered the existence of tender spots in the course of the median and radial nerves (branches of which had been injured) which did not exist on the uninjured side. He says it may be objected that the phenomena observed in these nerves were induced by the tetanic disease of the cord, and not by a pathological condition in the nerve stems. If that were the case, he continues, then they would be demonstrable equally well on the right side. The Professor makes the occasion one for a lecture on “The Diagnosis and Pathology of Neuritis;” and, with regard to the case of tetanus, concludes that there was no neuritis, and that the phenomena observed in the two nerves during life must be referred to an increase of irritability, &c., for the *post mortem* revealed that to the naked eye nothing abnormal was to be seen in the nerves, neither did microscopic examination in fresh and hardened preparations show anything that could be referred to neuritis or perineuritis.

It is evident, then, from this case—and probably others are recorded—that a difference between the injured and sound side has

been observed. In my case, however, the wound being in the foot, and the increased rigidity, &c., in it and the leg, in addition to a favourable termination, an opportunity presented itself of observing the gradual return of locomotive power which, as I have already stated, was comparatively rapid in the uninjured, but slow in the injured leg. I had not read Professor Nothnagel's paper while the case was under my care. Had I done so I would have repeated his experiment in order to ascertain if tenderness existed in the nerves. I regret having lost the opportunity of acquiring some information, corroborative or otherwise, on this point.

3. Among the numerous medicines used in the treatment of tetanus, chloral is one of the latest, and appears to possess considerable advantages. Certainly in this case, as I have already stated, the good effect was at once made evident. In *The Boston Medical and Surgical Journal*, June 24, 1875, there is an account of a large number of cases, all French, treated by chloral, with most satisfactory results. In other journals, too, there are numerous cases.

Bauer, in his article on Tetanus, in Von Ziemssen's "Cyclopædia," states that "other remedies are practically and properly being more and more supplanted by chloral hydrate. This is superior to every other means, even the inhalation of chloroform, for the accomplishment of the purpose in hand." After reviewing the various drugs which have been used, he goes on to say:—"The control of the spasm will be attained most satisfactorily by the use of chloral and bromide of potassium." The superiority of chloral, given by the stomach over chloroform inhalations, is accounted for by one writer (M. Pugliere) thus:—"The chloroform expends its force chiefly upon the brain, whereas chloral which has been administered by the stomach, after it reaches the circulation, sets free chloroform, which thus acts on the entire nervous system."

Billroth also states that hydrate of chloral has been given with some success in tetanus, and that several cases have recovered.

Liebreich asserts that strychnia is an antidote to chloral. If this be so, then we may surely give chloral in tetanus, and hope for a good result. Opinions differ as to the true pathology of the cord in tetanus; and although this is a part of the subject I do not propose to enter upon, I should like to remark, while on the subject of treatment, that if hyperæmia of the cord and its membranes exists—which is the opinion of most pathologists—it would not surprise me if treatment by belladonna, more especially if given early, were to produce satisfactory results.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Disorders of the Urinary Organs. By REGINALD HARRISON, F.R.C.S., Surgeon to the Royal Infirmary, Liverpool. Pp. 198. London: J. & A. Churchill.

MR. HARRISON has here brought together many communications made to medical journals upon the subject of urethral stricture and other disorders of the urinary organs. The greater part of the book is devoted to the various abnormal states of the urethra, and the effects of obstructions in that passage upon the other parts of the genito-urinary system.

In cases of stricture of long standing a pouch is formed behind the narrowed part, and this often remains even after the obstruction has been removed, allowing urine to lodge there and become decomposed. The draining of the urethra at night by means of a catheter slowly withdrawn, removes the urine, which otherwise becomes decomposed and is very foetid when voided in the morning. Occasionally we meet with cases in which the bladder-wall is not hypertrophied, but rather thinned. What determines the one condition or the other is probably little more than surmise; but when it is dilated and thinned, Mr. Harrison believes that this is caused by the bladder having gradually yielded to an obstacle which has been beyond its power to overcome.

A case of rupture of the bladder is reported in which it is presumed that the viscus burst in an effort to conquer a spasmodic stricture. The man was admitted in a state of collapse, and died in eighteen hours. A catheter was introduced immediately after his admission, "but only a few drops of blood-stained fluid escaped." At the *post mortem* examination a rupture of the bladder was found in the posterior wall communicating with the cavity of the peritoneum. It is supposed that there was a spasm which could not be overcome by the bladder, for the ureter was found to be of normal calibre. We do not think this a satisfactorily proved diagnosis. We do not know what process of catheterism

the patient was subjected to before he came into the hospital—for we are not told. But it is very probable that some attempt was made to empty the bladder before that. We see quite enough of the effects of rough operating with the catheter to believe that the rupture was probably done with an instrument. The wonder is that so many people escape from the hands of incompetent persons with only the unhappy condition of a riddled urethra instead of a perforated bladder. We have seen several cases of retention treated as resulting from stricture, while the urethra admitted a full-sized catheter at the very first attempt to introduce it. We know of a case of simple atony in which a general practitioner tapped the bladder above the pubis, with death as the result, although the urethra was of normal size, and a full-sized catheter was, by other hands, passed without a hitch.

In the treatment of stricture the author places first the most general method of gradual dilatation combined with rest. We agree with him in his warnings as to the danger of catheterism and the care with which that proceeding should be carried out. The blundering and recklessness which one sees is appalling. But, on the other hand, we cannot agree in the advice of Mr. Callender, quoted approvingly, that the operation should cease if bleeding occurs. This is quite true if the bleeding is the result of wounds made with the point of a bougie, but there are strictures which cannot be touched without some bleeding occurring, and such a rule would preclude the possibility of dealing with them altogether.

Mr. Harrison devotes a lecture to a case of urethral fever. The subject is of great interest, and much remains to be written upon it. The suddenness of onset in cases that appear the most unlikely, and the rapidity of a fatal result, make this disease the most formidable complication which the surgeon has to deal with. Patients die without an apparently sufficient cause beyond the passing of a catheter. The author holds the opinion that the fever is produced by nerve-shock; and, as bearing out that view, he mentions that there was an almost complete absence of these symptoms in cases in which anæsthetics were used for catheterism. "There is no character of pain so likely to produce shock as that which is the result of tension, as when a tightly-fitting instrument is slowly forced through a strongly resisting contraction." As to treatment the author finds the best remedy in aconite and quinine—the former being almost a certain prophylactic when administered in two-minim doses of Fleming's Tincture, immediately after

catheterism, as recommended by Long. For the suppression of urine, dry cupping and a teaspoonful of infusion of digitalis every hour or two (Gourley) are probably the best remedies.

Amongst external urethrotomies we observe that Mr. Harrison gives much prominence to the method devised by Mr. Wheelhouse. It is not sufficiently known. We regard it as one of the most excellent additions to urethral surgery. Mr. Harrison himself devises an internal urethrotome, double-edged, and uses oval-shaped bougies, by means of which dilatation of the urethra in a lateral direction is specially maintained.

A few lectures on Stone, Enlarged Prostate, &c., bring the book to an end. It does not pretend to be an elaborate treatise on the subject of which it treats, but rather a statement of personal experience of success and failure. The style is pleasing, and of the lighter kind of professional reading; and although it is not a book which will serve students much, there are many things in it which are suggestive to the surgeon and most useful in practice.

St. Thomas's Hospital Reports. New Series. Edited by DR. PAYNE and MR. MACCORMAC. Vol. VIII., pp. 696. London: J. & A. Churchill.

THE present volume consists of eighteen papers, besides the medical and surgical Reports compiled by the Registrars.

Dr. Peacock is the first contributor. His paper is an analysis of 92 cases of chorea which have come under his care since 1863; 26 of these occurred in males, 66 in females. The extreme limits of age were 27 and 7, and the mean age is almost alike in both sexes, being about $12\frac{3}{4}$ years. In 23 cases no cause could be assigned; in 25 there was a history of fright; in 6 there had been falls on the head; and in 9 rheumatism is set down as the exciting cause; 54 were reported never to have had any form of rheumatism, and in 24 rheumatic symptoms had been observed at various times before the onset of chorea. There were decided signs of heart disease in 14 cases. Dr. Peacock's experience does not agree with that of Sir Thomas Watson, who believed that chorea was more common in children having dark hair and eyes than in those of a fair complexion. What is more important is the fact that out of the 92 children only three or four could be said to be well grown, healthy, and well nourished. Rheumatism, he observes, seems to give rise to chorea rather by directly affecting the brain and spinal

cord, or their membranes, than by causing heart affection, and so secondarily involving the nervous system. There were 3 fatal cases, 15 relieved, and 74 cured.

There are three papers on the Use of Salicin, Salicylic Acid, and Salicylate of Soda. Dr. Sharkey believes the last to be much more effectual than the other two. The dose given was 20 grains every two or three hours. Out of 150 cases of acute rheumatism treated by Dr. Jacob by salicylate of soda, a markedly good effect was noticed in 103 cases, 42 could hardly be said to be benefited or the reverse, and in 5 the effect was unfavourable. In no instance was delirium caused by the drug, although a sort of nervous irritability, restlessness, and rapid breathing appeared occasionally to be due to it. Dr. Theodore Acland, in an excellent communication on the same subject, states that the quantity of urine is diminished directly or indirectly by the drug, that the percentage amount of urea excreted remains unaffected, but that, of course, the total quantity of urea excreted is considerably lessened. This fact, in his opinion, explains the failure of the salicylic treatment in enteric fever.

The most interesting paper in the volume is Dr. Ord's, on the Use of the Graduated Bath in the Reduction of Febrile Temperatures. Out of 60 cases of enteric fever he tried the graduated bath (by which is meant a bath of from 90° to 100° Fahr. cooled down in 20 or 25 minutes to about 70° Fahr.) in 13, and in all with satisfactory results. "Of the cases in which the baths were used, several were clearly arrested in a fatal course. That the bath was not always successful was to be expected. The lesions of enteric fever are so often sufficient of themselves to kill, that a remedy which deals chiefly with hyperpyrexia cannot be expected to be a panacea. So far, however, as the danger in any case is purely one of hyperpyrexia the efficiency of the bath is manifest. The cases in which it succeeded were cases of such a kind; the cases in which it failed were fatal by reason of peritonitis, perforation, or great extent of intestinal glandular lesion with exhausting diarrhoea. Even in these cases the bath has appeared to protract the struggle, affording delay, during which more favourable local states might be established. And it may be noted that another external application—the ice bag—has alone, or with the bath, been able to control violent peritoncal inflammation associated with symptoms almost certainly demonstrative of perforation." The rule adopted is that it should be used whenever the temperature, having reached 105°, is still rising,

particularly if the rise be before or after the usual evening hour. Dr. Ord believes that the graduated bath has a double remedial action—viz., the diminution of bodily heat in a direct fashion by the simple ablation of excess, and also by reflex nervous influence; as a soothing agent it controls internal and local inflammations.

Mr. Nettleship has published a series of observations in *Ophthalmic Practice*. Some of his cases bear out Mr. Hutchinson's opinion—that disorganising changes in the vitreous are amongst the occasional consequences of sexual exhaustion.

Transactions of the Bristol Medico-Chirurgical Society. Vol. I., pp. 140. Bristol: Kerslake & Co. 1878.

THE Bristol Medico-Chirurgical Society have issued their first volume of Reports, comprising abstracts of papers read during the past four years. Some of these papers are well worthy of a permanent place in medical records, and the volume, as a whole, by no means requires the apologetic tone of its somewhat rambling preface. Dr. Spencer has contributed an exhaustive memoir on the Chemical and Therapeutical Differences between Salicin and Salicylic Acid. He combats the theory of Senator of Berlin, who tried to show that salicin, as soon as it enters the stomach, breaks up and yields glucose and saligenin, and that the latter is oxidised into salicylic acid. He adduces several cases as clinical evidence justifying his scepticism of Senator's oxidation process. Dr. Fyffe's observations on Sunstroke are valuable and practical. In his experience he has found that it was not upon bright days that cases of sunstroke generally occurred, but rather during cloudy, moist weather, with little movement of the air, even though the temperature was not so high—a circumstance which he believes explains the greater frequency of insolation on the continent of America than in the West Indian Islands. The most interesting paper is one on the Pathology of Tetanus contributed by Dr. Long Fox. After mentioning the *post mortem* appearances noticed by Lockhart Clarke, Dickenson, and Clifford Allbutt, he himself has found (1) areas of cord softened, so as to be almost diffuent; (2) hæmorrhage outside the spinal dura mater, and gummy-looking fluid beneath the arachnoid; (3) distension of the vessels and thickening of the membranes; (4) fissures in the posterior white columns; (5) softening of cord and many amyloid bodies in the gray matter; (6) creamy posterior columns, the softening seeming

to be composed of colloid bodies. The colloid masses of which he speaks, and which do not take the carmine dye, are probably, as Dr. Batty Tuke believes, degenerated nuclei of the neuroglia, and not the large homogeneous albuminoid exudations described by Dr. Bookey and Professor Auerbrecht. Dr. Long Fox believes that none of the lesions yet found can be considered as in any way causes of tetanus. It is more likely, he thinks, that the blood itself is in fault. In strychnine poisoning—a condition so analogous to tetanus—Dr. Harley has shown that the blood is incapable of absorbing oxygen in the usual proportion. The immediately fatal phenomena of many cases of tetanus seem to point to some such explanation. This abnormal blood imperfectly nourishes the cord, and either thus, or by its distinctly toxic effects, diminishes the resistive power of that organ—in other words, a cord so conditioned is abnormally impressible. This impressibility renders arterial spasm morbidly facile, whether the exciting cause is the circulation in the cord of more of the morbid blood, or reflected irritation from a diseased nerve at the periphery, or reflex irritation from any other cause and from any other point in the body; and if this arterial contraction goes on for any protracted period, or is frequently repeated, we may find various lesions due to imperfect blood supply, in addition to those due to diminished nutrition from the original nature of the blood, whilst, as a sequence of the spasmodic arterial contraction, we may get dilatation, hyperæmia, and, perhaps, exudation; and lastly, the pressure of the exudation, or some peculiarity in its nature, may lead to further disintegration of the nervous centre.

Dangers to Health. A Pictorial Guide to Domestic Sanitary Defects.

By T. PRIDGIN TEALE, M.A., Surgeon to the General Infirmary at Leeds. London: Churchill. 1878.

It is a matter of no little difficulty to review a work when the illustrations exceed the letterpress. This work is what it professes to be, a "Pictorial Guide." The pictures are excellent, and the short descriptions accompanying each illustration are equally good and to the point. The illustrations include all the ordinary sanitary defects met with in dwelling-houses, and suggest effectual remedies for the same. The book will be particularly useful to medical officers of health in remote districts, to whom it will afford a ready means of graphically pointing out to the members of sanitary

authorities and their employes the best means of remedying sanitary defects, which are well-nigh universal. We commend the work to the Medical Officers of Health of Ireland as a useful weapon against the ignorance of sanitary authorities and the incompetence and dishonesty of builders' workmen. It also contains many useful hints to those who are as yet novices to the duties of medical officers of health.

T. W. G.

The Irish Medical Directory for the Year 1879. Dublin: Offices of THE MEDICAL PRESS AND CIRCULAR. London: Baillière, Tindall, & Cox. 8vo, pp. 619.

THE issue for 1879 of this annual publication is enlarged to upwards of 600 pages. Amongst the additions are "The Dentists' Act" (41 & 42 Vic., cap. 33), which received the Royal Assent on July 22, 1878; the "Public Health (Ireland) Act, 1878;" the new Militia Medical Regulations, and the Dental Regulations of the Royal College of Surgeons, Ireland.

This work is now almost indispensable to the Irish medical practitioner, and we congratulate the Editor on the success which has crowned his efforts to compile a thoroughly reliable Medical Directory for Ireland.

Impending Medical Legislation. Observations of the COUNCIL OF THE IRISH MEDICAL ASSOCIATION. February, 1879. Dublin: J. Atkinson & Co.

THE Council of The Irish Medical Association, having had in view the probability that, during the present session of Parliament, measures would be submitted for the alteration and amendment of the laws which affect the medical profession, issued a circular in February containing their views upon the principal points which were likely to be dealt with in those measures.

The Council point out that any proposals to alter, amend, or supplement the existing laws must, of necessity, take cognisance of the following matters:—

1st. The education of persons seeking admission to the profession.

2nd. The tests of fitness for admission to the practice of the profession.

3rd. The constitution of the authority which is to control the education and testing of candidates for admission to the profession, and which is to regulate generally the conduct of the teaching and examining bodies, as well as of the members of the profession.

4th. The powers and privileges of existing medical colleges and corporations, and their relations, on the one hand, to candidates for entry to the profession, and, on the other, to the controlling authority.

All the above matters require to be dealt with by a comprehensive scheme of legislation.

The Council are fully aware that the professional education of candidates for the medical profession is not such as it should be, while the preliminary education of medical students is especially defective—in many instances of so low a class that it should debar the student from admission to *any* profession. Furthermore, that the tests for fitness to enter the profession are so unequal and so irregularly applied, that candidates rejected by one licensing body can obtain diplomas to practise from another; and that candidates who have successfully passed examinations for diplomas in medicine are rejected at examinations for diplomas in surgery, and *vice versa*.

The Council believe that a complete alteration of the constitution of the General Medical Council, now the controlling authority of the education and examination of candidates, is essential, inasmuch as the defects in education and examination which have been just pointed out could not have continued during the whole twenty years of the existence of the General Medical Council had that body been a properly constituted, vigilant, and influential authority.

The Council of the Irish Medical Association recommend that any legislative measures which may be enacted should include—

1. A revision of the constitution of the General Medical Council:

(a) By a more equitable distribution of seats amongst the licensing authorities.

(b) By widening the constituencies which elect the representatives of several of those bodies.

(c) By a more direct representation of the general body of the profession.

2. The grant to the Medical Council of complete control over the education and examination of candidates for admission to the profession.

3. The compulsory establishment of a single conjoint examination for each division of the United Kingdom, which examination

should be essential for admission to the licence or degrees of any of the medical authorities.

4. The enforcement of absolute uniformity in standard of examination, extent of curriculum, and amount of fees payable, required by each and all of the conjoint examining bodies.

In conclusion, the Council believe that if the above principles should be adopted in the measures which may come before Parliament, they will receive the general support of the profession and the public.

Cyclopædia of the Practice of Medicine (VON ZIEMSSSEN). Vols. VIII. and XIII. London: Sampson Low, Marston, & Co. 1878 and 1879.

VOL. VIII. of this work contains Diseases of the Chylopoëtic System, with chapters relating to Diseases of the Bladder and Urethra, and Functional Affections of the Male Genital Organs. We observe with pleasure that the translation of the important articles on "Diseases of the Peritoneum," and on "Diseases of the Supra-renal Capsules," has been made by Dr. Reuben J. Harvey, and we may be allowed to congratulate the translator on the lucidity and smoothness with which he has rendered into English the unusually difficult German of the originals.

Vol. XIII., which treats of the Diseases of the Spinal Cord and Medulla Oblongata, and is the work of Prof. Wilhelm Erb of Heidelberg, is one of the most valuable volumes of the series, from the exhaustive method in which it deals with the numerous and the obscure affections to which the parts in question are liable. After an introduction, both anatomical and physiological, the general symptomatology and therapeutics are discussed; the general pathological anatomy is passed over as not being at present in a condition to be made useful to the practitioner. The portion of the work which is on the *Special Diseases* is divided into two sections—the first on *Diseases of the Membranes of the Spinal Cord*, the second on *Diseases of the Spinal Cord proper*. The classification of the latter which is adopted is into two groups. The first group comprises the processes which occupy, or may occupy, the entire transverse section of the cord in a diffuse way. They include hyperæmia, anæmia, hæmorrhage of the cord, acute severe traumatic lesions, and slow compression of the cord. Then follow three diseases—concussion of the cord, spinal irritation, and spinal

nervous debility—in which we are aware of no anatomical changes, but are allowed to suppose at any rate fine disturbances of nutrition. Then follow inflammation of the cord (acute and chronic), simple softening, and that peculiar anatomical change commonly known as sclerosis in patches. In the second group those forms of disease which entail degenerative processes in the posterior columns, in the lateral columns, and the lesions which are limited to one lateral half of the cord, are treated of; also the acute and chronic processes confined to the gray anterior cornua. After these is described acute ascending paralysis—a form of disease still perfectly obscure; then come tumours, secondary degenerations and malformations of the cord, and, finally, a number of scattered facts, which, having an undoubted, though in many cases an obscure, connexion with the pathology of the cord, are granted a little space as material for the further development of the subject.

Statement of the Income and Expenditure of the Londonderry City and County Infirmary for 1878. Pp. 15.

THE above is the scarcely satisfactory title of what is really a concise report of the work done in a single year by one of the Irish County Infirmaries—institutions which are now on their trial, and which must stand or fall according as the verdict of public opinion is given for or against them.

The Medical Report for 1878 is written by Sir William Miller, Knt., M.D., Surgeon to the County Infirmary, and Physician to the Londonderry Fever Hospital. We are only sorry that this Report is so very brief, and that it does not occupy the principal place in the pamphlet before us.

According to the Report it appears that during the year 858 patients were under treatment, of whom 791 returned home cured or relieved, 27 died, and 40 remained in the hospital under treatment on the 1st January, 1879. A very large number of serious accidents were treated during the year, many of them requiring operative interference. Amongst the accidents were upwards of 50 cases caused by the explosion on board the Allan steamer "Sardinian," in the month of April; of the cases from the "Sardinian" only two proved fatal, one being a child of two years old, burned about the head—the other a sailor, who died ten hours after admission, from internal injuries. Sixty-four important operations were performed during the year in the hospital, and a

number of minor operations, all with successful results. The general mortality was a fraction under $3\frac{1}{2}$ per cent.

In the City Fever Hospital 61 cases were treated, of whom 49 were cured, 7 died, and 5 remained under treatment on 1st January, 1879.

The demand for nurses from the Infirmary for private cases is steadily increasing, and Sir William Miller has always received most satisfactory accounts, both from the patients' friends, and also from the medical men in attendance, of the kindness, skill, and attention, displayed by the nurses.

Three medical students have been attending the hospital during the year, receiving clinical instructions in the medical and surgical cases, and also practical knowledge in the compounding of medicine, under Sir W. Miller's own superintendence.

The foregoing facts indicate three important functions which are fulfilled by the Derry Infirmary—namely, the treatment and relief of the sick or wounded, the supplying of trained nurses throughout the country, and the supplemental instruction of medical students who may for the time being happen to reside in the vicinity of one of the county infirmaries. Surely no one is warranted in questioning the *raison d'être* of the infirmaries in the presence of facts like these.

Clinical Reports on Renal and Urinary Diseases. By WILLIAM CARTER, M.B., Univ. Lond.; Physician to the Liverpool Royal Southern Hospital. London: J. & A. Churchill. 1878. Pp. 291.

WE have read this volume with both profit and pleasure, and can highly recommend it as a record of much honest and painstaking work. The author disclaims for it any pretension to be a systematic treatise on renal and urinary diseases, but anyone who reads it will perceive that it embodies the result of most systematic observation and laborious analysis. The author modestly attributes much of the credit to the intelligent co-operation of the devoted, educated, and well-trained women who superintend the wards of the Liverpool Royal Southern Hospital; but it is plain that the work is that of one who had the subject thoroughly at heart, and who followed out these often most tedious cases to the very end. We quite agree with him in his reprobation of the estimation of urea by nitric acid. The number of errata is large, and, to show

that we have read the book carefully, it might be considerably increased, but this we expect a second edition will soon rectify. We consider it a most useful book for anyone interested in renal disease.

Papers on Gynocardia Odorata, from which the Chaulmoogra Oil is obtained. Compiled by R. C. LEPAGE. London: Trübner and Co. 1878.

CHAULMOOGRA oil has attracted at least a passing attention, chiefly on account of its reputed powers in alleviating, if not curing, oriental leprosy, and Mr. Lepage has in this pamphlet collected most of the accessible information concerning the chaulmoogra plant and the medicinal uses of the oil. The seeds and oil of gynocardia odorata are employed in India as alteratives and tonics, and the published evidence seems to show that the oil externally is a valuable local stimulant, and deserves further trial in chronic affections of the skin.

Elettromiopatìa: Scienza Nuova che cura il sangue e sana l'organismo. Dal Conte C. MATTEI. 1878.

THIS small volume is a curious medley of homœopathy and medical electricity, written in an amusingly dogmatic manner. We did not succeed in making out what the author means by his five varieties of electricity—viz., positive red; negative yellow; white; azure; and negative green; but his enthusiasm carries him bravely over every difficulty.

Metric Weights and Measures for Medical and Pharmacal Purposes. By OSCAR OLDBERG, Phar.D. Washington. 1878.

THERE can be little doubt that before long the simplicity and harmoniousness of the metric system will so force themselves upon the professional mind that the old and incongruous systems (!) of weights and measures must give way before it. Last year the metric system was officially adopted by the U. S. Marine Hospital Service for all medical and pharmacal purposes, and Dr. Oscar Oldberg, of that department, has issued in a very concise and easily intelligible form the rules requisite for converting apothecaries' weights and measures (U. S.) into their respective equivalents

in metric terms, together with suggestions for writing metric prescriptions, and also a metric posological table.

In expressing quantities by weight the terms *gram* (gm.) and *centigram* (cent.) only need be used; and in expressing quantities by measure the term *cubic centimetre* (c. c.) is alone needful. Surely there can be nothing simpler than this nomenclature, and it is high time that medical text-books should be brought into harmony with modern works on the collateral subjects of physics and chemistry.

We may add that copies of metric documents issued by a Boston Society can be had on application to the Editor, at the office of *The British Medical Journal*, 161, Strand.

Testimonies to the Efficacy of Hydropathy in the Cure of Disease.

Edited by RICHARD METCALFE. London: Tweedie & Co.

THE object of this publication appears to be to advertise Mr. Metcalfe's system and his hydropathic establishments, and the only portion of the book which is worth reading is the reprint of a lively article contributed by the late Lord Lytton to the *New Monthly Magazine* in 1845, and entitled "Confessions of a Water Patient."

Note on Hydrobromic Acid. By E. SQUIBB, M.D. Brooklyn. 1878.

DR. SQUIBB advocates a process for preparing hydrobromic acid from bromide of potassium and sulphuric acid, which he finds to be more convenient and to yield a purer product than Fothergill's method. The acid is on its trial as a sedative neurotic and as a substitute for the alkaline bromides, but a serious drawback to its administration in full doses is the degree of dilution rendered necessary by its sharp biting taste. A dose of 50 grs. (41·6 minims) of Squibb's solution, equivalent to 25 grs. of bromide of potassium, requires not less than ʒ viij. of liquid, containing at least an ounce of sugar in solution. Dr. Fothergill, however, employs the acid in smaller doses, and holds it in high esteem.

PART III.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

SESSION 1878-9.

HENRY H. HEAD, M.D., President.

GEORGE F. DUFFEY, M.D., Honorary Secretary.

Wednesday, February 5, 1879.

JAMES LITTLE, M.D., Vice-President, in the Chair.

DR. WALTER SMITH read a paper on "Illustrations of the Use of Chrysophanic Acid in Diseases of the Skin." [It will be found at p. 193 of the number of this Journal for March.]

DR. HENRY KENNEDY said that in cases of ringworm of the body, and in one case of ringworm connected with the scalp, he had found the use of the acid successful. If used too strong it created a good deal of irritation, particularly in children, and a condition resembling erysipelas accompanied with a good deal of œdema. The strength of the ointment he used was five grains to the ounce.

DR. GRIMSHAW said he had used the acid with success in a number of cases of tinea versicolor. He had altogether given up diluting the acid. He had had some hundreds of cases, and his practice had been to simply moisten the part and rub the acid over, and he had found that in nearly every case one application cured the part. The irritation varied very much and unaccountably in different persons. His practice had previously been—first to apply the acid to very small portions of the surface, and then, if the irritation was not considerable, to apply it to larger portions. He had found it effectual in cases of psoriasis. He tried it in one or two cases of ringworm of the scalp, but the patients disappeared before he saw the result. He thought the dangers to be apprehended from irritation were exaggerated. With respect to the use of agents applied to the skin with friction, they were aware that a great deal of harm was often done

by the friction ; and he thought that the fewer applications they had of any drug like chrysophanic acid the better. In some cases he had found one application of the pure acid to a moistened skin sufficient to cure tinea versicolor. He had experienced no more irritation from putting the pure acid upon the affected parts when moistened than from the use of the ointment in which the acid was considerably diluted. He had tried it mixed with lard, glycerine, olive oil, and various other things, and had found that the pure acid, without oleaginous material, used in the way he had stated, was the best.

DR. J. W. MOORE said that some months ago an elderly man, who had been the subject of inveterate and very general psoriasis, came under his care at the Meath Hospital. He had been under the care of the late Dr. Noble Seward, of Tallaght, who had treated him with arsenic, and prescribed for him judicious dietetic measures. The patient had been addicted to habits of intemperance, and was warned against this vice. After being treated he became intemperate again, and the disease returned. It was exceedingly general, the entire trunk and the upper and lower extremities being affected. Dr. Moore began the treatment with chrysophanic acid, and after two or three applications the patient became better. He commenced with an ointment containing 20 per cent. of the acid, beginning cautiously and applying it at first to only one extremity. The man was so pleased with the effect that he entreated a further supply of ointment, and went on applying it until ultimately all the psoriasis scales disappeared. At first the bed used to become filled with them in twenty-four hours. The patient left the hospital with his skin discoloured, but apparently quite healthy. He was under treatment altogether for six or seven weeks, and during that time used from ten to twelve ounces of the ointment.

DR. DUFFEY said it had been proved beyond doubt that chrysophanic acid was antiparasitical. Did it, however, possess any advantage over other parasiticides previously used, which were not attended with the disadvantage of causing staining of the skin or irritation ? As regards the treatment of tinea versicolor, he had frequently employed sulphurous acid and solutions of corrosive sublimate with success. He did not see the advantage of using a remedy simply because it was new, when they had older remedies which produced the same effect in as short a time.

DR. JOHN HUGHES said that he had used chrysophanic acid in various forms of scaly disease of the skin, including psoriasis, chronic eczema, and pustular eruptions of the skin, and in every one of them its temporary success was something wonderful—far beyond his experience of any other remedy. But he had yet to learn whether they could regard those results as permanent. One of the worst cases of psoriasis that had come under his observation was that of a girl of twenty-four, who had suffered for seven years from this disease without intermission. There was no

part of her body, from her scalp to her feet, that was not covered with scaly eruption. After the use of the ointment for some time her skin became perfectly clean, and she left the hospital, and, after a convalescence of three or four weeks, went to the country. He had heard from her since, but was not able to say whether the disease had returned or not. In cases of chronic eczema he had found the acid equally successful. A remedy looked rather like a quack one which had been applied to all forms of disease and had proved efficacious in each and every one of them; and therefore it seemed rather premature for them to pin their faith to it without knowing exactly the results they were likely to attain in the *modus operandi* of the remedy. In some of his cases chrysophanic acid had produced intense inflammation of the skin; and in the case of the girl he had alluded to, swelling of the face and eyelids and other marks of acute inflammation. He had used the acid in both the strong and mild form, and in each case it was efficacious. His experience of the remedy was very limited, and was altogether confined to the cases he had mentioned. In all of them it had removed the eruption, but whether the cures were permanent or not he was unable to say.

DR. VESSEY spoke of the efficacy of a strong solution of hyposulphite of soda (three ounces to a pint of water) in tinea versicolor.

DR. RUTHERFOORD KIRKPATRICK said that he had used chrysophanic acid in one or two cases of psoriasis, and in two or three cases of ringworm of the scalp in children. It cured the disease perfectly, without any irritation of the skin, and in a moderately short time. The individuals disappeared, and he did not see them afterwards.

The CHAIRMAN mentioned a case of psoriasis that he had treated with an ointment consisting of a drachm of chrysophanic acid to the ounce of vaselin, which produced a very irritating effect. In a case of psoriasis inveterata he was now trying chrysophanic acid ointment. According to the patient's (a young lady) own account, it succeeded better with her than any other preparation she had used, but it had not yet kept her free from the disease, which reappeared.

DR. WALTER SMITH, in reply, said that the stains of chrysophanic acid only lasted two or three days, and the removal of them was assisted by the use of benzole. As to the effect of alkalies in producing colour, it might be mentioned that if a piece of moist soap were rubbed down the back of a patient, which had been treated with chrysophanic acid, a vivid red colour would be developed; and if it was glycerine ointment that had been used, the colour would be still more vivid. Dr. Grimshaw's simple mode of using chrysophanic acid was a reversion to the old way of using goa powder, which was to moisten a little of it with water and apply it to the part. He (Dr. Smith) only meant by his paper to show that chrysophanic acid was likely to be of utility in a certain class of cases. Corrosive sublimate should be cautiously used, especially with persons who

had tender skins. Of course they could not expect any remedy to prevent the recurrence of a parasitic disease.

DR. SMYLY read a paper on "Two Cases of Hystero-Epilepsy." [It will be found at p. 200 of the number of this Journal for March.]

DR. SIGERSON said the contribution of Dr. Smyly was of peculiar interest, not only on account of its intrinsic merits, but because it was, he believed, the first report of cases of hystero-epilepsy given in this country at any length. It might well be that the disease was not so exceptional in its appearance as some had supposed, and the present description bore out the opinion of Professor Charcot who, in a recent conversation, had stated his belief that it would be found to exist in these islands. It had, however, been brought into prominence but lately, and its name had not been received without considerable demur. In the cases related by Dr. Smyly the mental faculties were shown to remain intact in the intervals between the paroxysms; and this was the rule in hystero-epilepsy, even when of old standing, but was not the case where true epilepsy of comparable severity was present. There were some anomalous phenomena in the account given, and as their acquaintance with the domain of the disease increased, they would, doubtless, have many anomalous cases. These should be classed apart for a time, until the laws that ruled them had been deciphered. It was a great mistake to take, as some had done, an anomalous case, and reason, from this particular instance, against the reality of general laws deduced from the careful examination of a large number of cases. With respect to the question of metallotherapy, advocated during many years by Dr. Burq, it was divisible into external and internal. The latter was still under judgment. There could, however, be no doubt as regards the efficiency of the external application of some metals in causing the disappearance of "permanent" hysterical phenomena, such as hemianæsthesia, achromatopsia, and others. He (Dr. Sigerson) had witnessed the effect that followed such applications at La Salpêtrière on very many occasions, and felt proud to state that the conditions were such as to preclude the operation of expectant attention. In two instances, at least, the patients were not victims to hysteria, as the hemianæsthesia had supervened on well-defined brain lesions. He had, besides, had the advantage of assisting at some original investigations, undertaken by Professor Schiff, in which the utmost precautions had been taken to guard against error. No word was spoken to lead the patient to anticipate what was to follow, nor could she have obtained information in any way, inasmuch as the result was one quite unexpected. A portion of the table at which she sat was screened off, so that, during the electric experiments, contact could be made or broken without her knowledge. When the small solenoid is placed on a finger of the anæsthetic hand, feeling returns. When it

was so placed here, and the patient kept in a state of expectancy, feeling did not return in the prescribed time. The reason evidently was that, during this period, Professor Schiff had kept the circuit open; for, when it was closed afterwards, the feeling returned. Again, the patient was made to breathe through a paper tube, which entered the cavity of a large concentric bobbin. So long as the circuit was kept open there was no effect, but when it was closed the hemianæsthesia disappeared from the side affected. Commonly there is metastasis or transfer, but when they expected to find the hemianæsthesia shifted to the other side they found, to their surprise, that the patient was quite free. The achromatopsia of the right eye had likewise vanished without reappearing in the left. The explanation of this total disappearance seems to be that the breath passing through the cavity of the large solenoid, and electrically affected by it, influenced both sides of the body. This experiment had been suggested by the effect produced on a patient suffering from sacral neuralgia, in whom the pain returned whenever he breathed through the solenoid. Finally, when several pieces of metal are placed upon a surface of anæsthetic skin, and one of them proves efficient, or when a zone of sensibility develops opposite the poles of a magnet placed behind the patient's back—or when, as here, results occur which could not have been anticipated, the operation of expectant attention must be regarded as excluded.*

DR. HENRY KENNEDY spoke of the value of mercury in small doses, as prescribed by the late Dr. Law, in cases resembling those described by Dr. Smyly.

The CHAIRMAN said he would be glad if Dr. Smyly would mention whether he thought the illness of which the young lady died was a development of the ailment from which she had so long suffered, or some illness which she had accidentally contracted, such as typhoid fever or the like, for it was a well-established rule, with few exceptions, that, in the presence of organic disease, hysterical phenomena disappeared. This was a valuable rule in difficult cases. When well-marked hysterical phenomena appeared, the probability was that there was no disease of the lungs or any other part of the body. He was quite aware that patients who showed hysterical phenomena occasionally did die of other diseases. Dr. Wilkes recorded a case of a hysterical patient of his who died, and he was permitted to make a *post mortem* examination, when he found an entire absence of any organic disease that could have killed the patient. It would appear that, in Dr. Smyly's case, death might have resulted from some abdominal mischief or other accidental ailment—or was it a gradual development of the disease from which she had previously suffered?

* A full discussion of this question will be found in the *British Medical Journal*, Feb. 1, 8, 1879.

DR. FRANKS said he saw the second of Dr. Smyly's cases. The phenomena were very extraordinary, but he was perfectly convinced that they were *bond fide*. It was remarkable that the moment the mesial line was passed anywhere she immediately winced.

DR. SMYLY, in reply, said that the first case he had dealt with was seen by his brother, Dr. P. C. Smyly, and by Dr. Kidd and Dr. J. W. Moore, in 1877; and the second case was seen by Dr. Franks and a great many students of the hospital. He did not mean to convey that the patient in the first case died of hysteria. On the contrary the facts bore out the rule mentioned by the Chairman, for the symptoms of hysteria disappeared for a fortnight before her death, and she died from the effects of diarrhoeal vomiting, brought on, he believed, by fasting for so long a period. During three or four years she had taken only one meal in the twenty-four hours, and vomited almost immediately afterwards. He had known hysterical patients to starve themselves, and in this case he ordered food to be left by her bedside at night in order that she might have an opportunity of taking it, but her relatives told him that she never did so. The second case also bore out what the Chairman had stated. The girl came to him on the 24th of January in the condition described, with constant protrusion of the tongue. She had no anæsthesia, but simply Bell's palsy. She afterwards went into the Mater Misericordiæ Hospital, where she was under Dr. Hughes' care. For some reason or other she left it; and, on her way home, on an excessively wet day last week, got congestion of the lungs, and the protrusion of the tongue and other hysterical symptoms then disappeared.

DR. HUGHES said that the patient referred to as being in the Mater Misericordiæ Hospital suffered from well-marked chorea in the upper and lower extremities for several years. She got married, and he lost sight of her; but before that he had seen her, and the muscular action was then entirely confined to the tongue.

The Society then adjourned.

Wednesday, March 5, 1879.

JAMES LITTLE, M.D., Vice-President, in the Chair.

Note upon Mitral Stenosis. By CHRISTOPHER J. NIXON, M.B., Univ. Dubl.; F.K.Q.C.P.; Physician to the Mater Misericordiæ Hospital; Lecturer on Institutes of Medicine in the Medical School of the Catholic University.

THE heart which I have the honour of exhibiting is, I think, associated with features of sufficient clinical interest to warrant me bringing the case before the Medical Society of the College of Physicians. It was

taken from the body of a young girl, aged twenty, who was admitted into the Mater Misericordiæ Hospital upon the 16th of last month. The patient gave a history of having suffered some years before from rheumatic fever with cardiac complication. Since then her breathing was short upon exertion, but she was not troubled with cough. A few days before she came to the hospital she complained of sore throat and pains in her legs and arms. When I first saw her she had considerable enlargement of both tonsils with general catarrhal inflammation of the throat. She had pains in her knees and ankles, but no swelling of any of the joints. On examining the heart, I found that the apex beat was displaced towards the left side, and that it was preceded by a marked presystolic thrill. Upon auscultation, whilst the patient was up and dressed, I heard a very well-pronounced presystolic murmur of the usual acoustic character. A few days afterwards the patient complained very much of the arthritic pains, and the ankles became swollen. Her pulse rose in frequency, and she presented the general aspect of one getting some sharp pyrexial attack. Upon a careful examination of the lungs, I could detect no evidences of disease. The murmur over the mitral area seemed to be somewhat altered in character. It might be described as occupying the entire of the long pause, and consisting of two different elements—one immediately following the second sound of the heart, the other preceding the first sound. Of the two elements the first was perhaps the more pronounced one, and my attention was specially called to it by my resident pupil, Mr. Lehane, who, when examining the patient in bed during the previous day, thought that the mitral murmur was more closely associated with the second cardiac sound than with the first. The two portions of the murmur were not quite similar in acoustic characters; it seemed as if its cadence fell in the middle of the long pause, being most developed at the initial and terminal portions, and somewhat rough in the latter. It was terminated by the sharp-clicking first sound, which was succeeded by a faintly-pronounced second sound. The sound of Skoda was exceedingly well marked, and occasionally replaced by the *bruit de rappel*, although the patient, singular to say, complained of no cough, and never had hæmoptysis.

In studying the features of the case with the hospital class, special attention was directed to the existence of the post-diastolic murmur, and to the sharp clicking character of the first sound; and having regard to the value attached to these signs by different observers, the diagnosis was made of an *extreme* amount of mitral narrowing with a funnel-shaped as opposed to a diaphragmatic condition of the mitral valve.

The patient's disease ran what I may term a tumultuous course. She got repeated rigors; her temperature rose to 102° F.; the pulse varied from 110 to 130, being regular though weak; the respirations were 24 in the minute. A considerable increase in the area of cardiac dulness

existed towards the right side, but there were no evidences of venous engorgement, either in the neck or in the hepatic region. The patient suffered very much from irritability of stomach, and a mixture of bicarbonate of soda with cherry laurel-water and infusion of calumba was given in effervescence. Twelve minim doses of the tincture of digitalis were added, the ice-bag was kept over the heart, and iced beef-tea, milk, and wine, were given at short intervals.

On the morning of the 23rd of February I found the patient considerably worse in all respects. The vomiting, which had abated somewhat, was now incessant, the matter discharged being of a light greenish colour. Her pulse was 140, weak but regular; her face was flushed to a dusky hue; the rigors had recurred with great frequency, and she complained of acute pain over the region of the heart and spleen. She had passed a restless night, wandered at intervals, and with difficulty could be kept in bed. Upon examining the heart I found the murmur still audible and the presystolic thrill existing as before. There was great hyperæsthesia of the entire cardiac region, so that the patient could scarcely bear the lightest pressure of the stethoscope. Her manner was extremely excitable and restless; she spoke hurriedly, and manifested great impatience at being kept in the hospital. She wished to go home at once; she knew if she got to her native air she would get well, so that it was with some difficulty we succeeded in getting her into a quiet mood. The administration of digitalis was stopped, small doses of hydrocyanic acid were ordered, and a blister was directed to be applied to the epigastrium, but the vomiting continued unabated, and the patient could only take ice and iced wine in small quantities. Diarrhœa of an involuntary character set in; the tongue became somewhat brown and dry, though clean; a roseolar rash appeared upon the legs and arms; the skin assumed an icteroid hue; the delirium became violent, and towards the evening of the 24th merged into coma and death.

The *post mortem* examination was necessarily of a limited character, and I could only obtain the heart and lungs, removed through a small opening in the thorax, which was concealed by the opiate plaster applied over the cardiac region. I know nothing therefore as to the condition of the spleen or kidney, though the acute pain complained of in the region of the former would indicate the occurrence of splenic hæmorrhagic infarction.

Both lungs were somewhat congested posteriorly, but not to the extent one would expect from the cardiac lesions which existed. They were emphysematous at their anterior margins; the lowest lobe of the right lung presented two or three spots of hæmorrhagic infarction, situated at the surface, and of the usual appearance and form. One of these is exhibited to the Society.

The heart and pericardium weigh fifteen ounces. The specimen

illustrates very beautifully the complete obliteration of the cavity of the pericardium, resulting from a long antecedent inflammation of the membrane. There is a remarkable absence of any great degree of thickening—a point most important to note in explanation of the hypertrophy or atrophy of the heart which follows adherent pericardium. The greatest amount of compression seems to have been exercised upon the auricular appendices. The cavity of the left ventricle appears to be of normal size, and its walls of normal thickness. As will be seen from the specimen, the curtains of the mitral valve form a funnel with the apex downwards. The orifice between the extremities of the valve-flaps does not admit the tip of the little finger, though it is now much larger than when removed from the body, as owing to repeated examinations a portion of the fibrin which was deposited upon the edges of the valves has been removed. The calibre of the aorta seems to be diminished; the left auricle is very capacious, its walls are hypertrophied, and its lining membrane greatly thickened and rugose. The left auricular appendix may be observed to be partly filled by a friable thrombus; the right ventricle and auricle are dilated, and the walls of the former are greatly hypertrophied. Small portions of decolorised fibrin may be observed entangled amidst the muscular trabeculæ. The endocardium is in parts opaque and thickened, and that portion of the membrane which forms the edges of the tricuspid valves presents a very beautiful example of recent inflammation, closely resembling that condition met with in cases of chorea, to which the name of chorea heart has been applied by Broadbent.

The case was a markedly acute one, and in its course it very closely resembles the conditions which, according to Rosenstein, are developed in the pyæmic form of acute diphtheritic endocarditis. Repeated rigors, delirium, abdominal complications, and coma, each following the other in rapid succession, are signs not commonly met with in inflammation of the serous membrane. Usually in chronic valvular lesions of the heart the fatal result is brought about by slowly developing mechanical disturbances of the circulation. In many such cases, however, the disease often runs a paroxysmal course, and I believe that the recurrent orthopnoea, the restlessness, the accelerated pulse, and the increase in size of the heart, can only be explained by assuming the occurrence of a freshly lit up endocarditis. It is especially in the chronic heart lesions of rheumatism that such paroxysmal phenomena are developed, and death sometimes unexpectedly results from the effects of the inflammation either directly upon the myocardium or upon the cardiac ganglia. The existence of a changed form of structure such as a sclerosed valve seems to be a predisposing cause for accessions of inflammation.

My chief object, however, in bringing forward this case is to call attention to the physical signs of mitral stenosis which existed. Of all valvular lesions of the heart that of mitral narrowing is, I consider, the

one most readily determined, assuming the special signs associated with it are present. No doubt the lesion may exist with a systolic apex murmur, or with merely a sharply-defined first sound. In such cases the diagnosis of mitral stenosis can be only presumptively made. What seems, however, to be certain is that if presystolic murmur occurs at any time in the course of cardiac disease, the existence of mitral stenosis can be infallibly predicated.

Having then an indication of pathognomonic value of this condition, it may seem an unnecessary refinement to multiply signs, if the introduction of a new one be not sufficiently distinctive, or if it have not a different import from the older one.

Dr. Hayden is, I believe, entitled to the credit of directing special attention to a murmur occurring in the initial part of the long pause, which he terms a post-diastolic murmur. It is held to indicate an *extreme* amount of mitral narrowing. The explanation offered of its production is that the great obstruction to the passage of the blood through the narrowed mitral chink causes such an extreme amount of tension of the auricle and of the veins opening into it that immediately when the valves are opened an eddy is formed in the blood-current which generates a murmur. Theoretically, I presume, this murmur occupies the entire of the long pause, falling in cadence in the middle, and becoming intensified and altered in pitch during the systole of the auricle.

In reference to this point I may say that I think the association of the terms systolic and diastolic murmur with the sounds of the heart objectionable and confusing, at least for the diagnosis of lesions of the auriculo-ventricular valves. It would, I believe, be more correct to connect the murmurs developed at the auriculo-ventricular openings altogether with the first sound of the heart. Taking, for example, the left side of the heart, we might recognise three murmurs of positive value developed over the mitral, and three over the aortic areas; at the former the presystolic, the systolic, and the post-systolic murmurs; at the latter the systolic, the diastolic, and the post-diastolic murmurs. To each and every one of these murmurs a fixed value can be attached—a value not merely hypothetical but of absolute importance. I think this classification would tend to simplify the signs of cardiac disease for students, and obviate some of the confusion which so often exists as to their value. If presystolic murmur, an unequivocal sign of mitral stenosis, be associated with a particular condition of the left ventricle, or with the first sound, why associate another murmur which indicates merely an exaggeration of the stenosis with a different condition of the ventricle or with the second sound? It would be apparently more consistent to speak of two forms of presystolic murmur—one, the short presystolic, developed immediately before the first sound; the second, the long presystolic, occupying the entire of the long pause or the initial portion of it.

It is for this reason that whilst I think the sign of extreme mitral narrowing indicated by Dr. Hayden is of much value, still following the aphorism *entia non sunt multiplicanda præter necessitatem*, I would prefer in this particular instance the qualification of an old and very suggestive term to the introduction of a new one.

Another point in the case to which I desire briefly to allude was the pure sharp click of the first sound, abruptly terminating the murmur. This is very commonly met with in mitral stenosis, and it has led not infrequently to the existing murmur being taken as systolic—the second sound in such cases being very feebly pronounced. According to some observers a clicking pure first sound indicates a funnel-shaped condition of the mitral valve, and Dr. Silver holds that in such cases the sound is produced by the contact of the flaps of the valves “by surfaces, not edges.” Anyone looking at the specimen submitted to the Society will, I think, find some difficulty in appreciating this view. It occurs to me that where mitral stenosis has been brought about by a chronic inflammation which causes a cohesion from above downwards of the borders of the valve-flaps a funnel-shaped valve is produced. The contraction of the muscoli papillares is sufficient to bring the edges together; there is then no murmur of regurgitation, but the thickened valve-flaps produce by their tension a pure and intensified first sound. On the other hand, where stenosis is the result of acute inflammation, as in ulcerating endocarditis with subsequent contraction, or where there is a deposition of fibrin upon the valves, the constrictive lesion is associated with a murmur of regurgitation.

There are other points of interest in this case which might have deserved some comment. My desire, however, in bringing it forward was simply to direct attention to the sign of extreme mitral stenosis which existed, and to the apparent value of a pure and intensified first sound in one of the forms of this valvular lesion.

The CHAIRMAN said the peculiar intensified first sound, described by Dr. Nixon, had been observed by himself in a young girl who suffered from very extreme mitral stenosis with pulmonary apoplexies. The loud sound following presystolic murmur seemed to him to be Skoda's sound—an emphasised pulmonary second sound.

DR. H. KENNEDY was at a loss to account for the cause of death in Dr. Nixon's case. Was it an example of any specific fever, or did death arise from the condition of the heart?

DR. NIXON, in reply, stated that he had not had any difficulty in determining that the sharp clicking sound was really the first sound of the heart. It should be borne in mind that the clicking first sound in mitral stenosis was followed most frequently by a feebly-pronounced second sound, and where this could not be heard, the time of the click

should be determined by ascertaining its relation to the impulse beat or carotid pulse. If the loud click represented the sound of Skoda, obviously the murmur would be systolic, not presystolic. With reference to Dr. Kennedy's question, he thought the cause of death was endocarditis, and that the fever which was developed was symptomatic of the local disease. As he had mentioned, the case very closely corresponded with one of the fatal forms of endocarditis described by Rosenstein.

Amyotrophic Lateral Sclerosis.

DR. NIXON exhibited a patient suffering from this disease of the spinal cord to the Society. The chief points noted were:—Bulbar palsy; extreme muscular atrophy of the upper extremities; motor paraplegia of lower limbs, with a spastic gait; an increase of tendinous reflexion in the ligamentum patellæ; spinal epilepsy and tremulation of the lower extremities, with perfect conservation of sensibility.

ON THE TREATMENT OF MORBUS COXARIUS BY A NEW METHOD.

DR. JOSEPH C. HUTCHINSON (*American Journal of Medical Science* for Jan., 1879, and *N. Y. Med. Record*) advocates the following plan of carrying out the indications for the treatment of morbus coxarius, which he considers to be:—(1) to secure immobility of the joint; (2) to procure extension of the limb; (3) to take off from it the superincumbent weight of the body; (4) to provide means to enable the patient to take exercise in the open air. He considers that immobility is obtained by the rigidity of the joint, and that this continues until nature says it is no longer necessary. To obtain extension of the limb and to remove the weight of the body, he resorts to the following device:—On the shoe of the sound limb an iron sole is applied, three inches high, so as to raise the foot from the ground. This elevated shoe and a pair of crutches constitute the apparatus. As the patient stands on his crutches the diseased limb is suspended. The shoe should be high enough to prevent the toes of the affected limb from touching the ground. By these simple means we fulfil all the indications for the mechanical treatment of hip-joint disease. Immobility is obtained and friction prevented in the manner above indicated—chiefly by rigidity of the periarticular muscles. Extension made by the weight of the suspended limb, which is greater than the weight ordinarily employed for extension, is quite sufficient to relieve the inflamed parts from pressure and pain, and to overcome deformity of the limb even though it be considerable; the weight of the body is removed from the diseased joint, and the patient can enjoy all the benefits of open-air exercise.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

FORTY-FIRST ANNUAL SESSION.

EDWARD B. SINCLAIR, M.D., President.

WILLIAM ROE, M.D., Honorary Secretary.

Saturday, March 1, 1879.

DR. RUTHERFOORD KIRKPATRICK in the Chair.

Ovarian Cyst.

DR. ATTHILL exhibited an ovarian cyst from a patient on whom he had performed ovariectomy in the Rotunda Hospital. The patient was a married woman, aged about thirty, the mother of three children, the youngest of whom was fifteen months old. The previous history of her case was very obscure. All he could make out was that she had nursed for nearly twelve months, that during that time she felt weak and lost flesh, and that menstruation did not return until after lactation ceased. After she had given up nursing her health did not improve, and on seeking medical aid for her increasing debility, a tumour was detected in the abdomen by the local medical man, who sent her to town. On examination he (Dr. Atthill) found a tumour, irregular in shape, occupying the lower portion of the abdomen; it reached nearly as high as the umbilicus, was markedly depressed in the centre, and simulated in its external features a fibro-cystic tumour. It was, however, quite separate from the uterus, which organ was of its normal size, and he had no hesitation in pronouncing it to be an ovarian cyst, and, after consultation, it was decided to remove it. The operation was performed on the 5th of February last, and, as far as the first stages were concerned, was exceedingly easy, the only drawback being that the patient retched several times. He came down on the peritoneum with the greatest facility, raised the peritoneum up in the usual manner, divided it, and slipped in a director; immediately on doing so, he was surprised to see a gush of chocolate-coloured fluid. One of the gentlemen who assisted him suggested that the fluid was peritoneal, but he (Dr. Atthill) pointed out that this could not be so, as there had been such resonance in both flanks, and that it must be from the cyst. He then passed in his finger and discovered a rent in the cyst; there were no adhesions, and it was drawn out without trouble, and the pedicle severed without the use of the clamp. The abdomen was then thoroughly cleansed

and sponged out, so that he did not think any of the fluid was left. The patient rallied in a short time and said she felt comfortable, but from the very first vomiting set in and she slept very little. The operation was performed about ten o'clock in the morning, antiseptically, under the thymol spray. At six o'clock she vomited green fluid, which, as it is seen so frequently associated with peritonitis, caused him much anxiety, still she felt no pain. Her pulse was at this time 104, and her temperature 101°. She was treated by hypodermic injections of morphia, which checked the vomiting for a time, but it returned, and next morning the fluid vomited was black like coffee grounds. The vomiting was held in abeyance by hypodermic injections of morphia, but never for more than a few hours. She was allowed nothing but iced water; and, in fact, took no nourishment. On the 6th of February, in eighteen hours, her pulse had risen to 125, and her temperature to 102°; the black vomiting continued, but at long intervals, because she was kept constantly under the influence of morphia. On the 7th, her temperature fell to 100·8°, and her pulse to 120, the coffee-coloured vomiting continuing; there was very little variation in the temperature—that of the axilla, on the evening of the 8th, was 100·1°. At this time she only vomited about once in the twelve hours, but it was always coffee grounds. One unfavourable circumstance was that, notwithstanding the fall in the temperature, the pulse kept up to 120 or 130; from this time the vomiting ceased. On the evening of the 9th, her temperature had fallen to 99·8°. She was then taking brandy and essence of chicken. Her temperature during the next twenty-four hours continued steady at 99°, and her pulse from 120 to 125. On the evening of the 10th her pulse fell to 116, and her temperature to 98°. During the whole of that day her temperature kept steady at 98°, until nine o'clock, p.m., when it rose to 99°, and her pulse to 130. On the 11th her temperature continued at 99° until evening, when it suddenly rose to 102°, her pulse being 130; she then for the first time complained of intense pain, and sank rapidly, and died in about six hours. The pain was relieved by hypodermic injection of morphia. She died on the seventh day after the operation. The steady fall of temperature and cessation of vomiting while the case ran on to a fatal termination were remarkable. On making an autopsy they found universal peritonitis, with adhesions everywhere. On examining the cyst it presented a remarkable appearance, a large patch of the wall surrounding the rent being of a much darker colour than the rest; it also appeared softer. Dr. Harvey, who kindly examined it, stated "there was a circle of about 2½ to 3 inches diameter on the surface of the cyst, which was discoloured, being of a darker brown colour than the rest of the cyst. In this area was a large opening into the cyst; the wall of the cyst here was very thin, was in one place divided into two laminæ, was rather softer than the other parts of the cyst wall, and appeared to be in a state

of commencing softening. The nature of the tear or opening suggested very forcibly the idea that it was a rupture—not a cut.” He (Dr. Atthill) had performed the operation of ovariectomy pretty frequently, and could not suppose that he had either cut into or torn the cyst without his knowledge. At the time he passed his director under the peritoneum he was not conscious of the most trifling resistance. His impression was that the cyst ruptured during the vomiting which occurred while the patient was coming under the influence of the ether, the wall of the cyst having commenced to soften previously. This was the only explanation he could give of what occurred. It was certain, however, that at one part the cyst wall was very thin, that softening had commenced, and that it was running on into decomposition. From this they learned that it was not always wise to postpone this operation too long. He had not hurried it in this case, because the cyst was still so small, and he probably would have postponed the operation but that all her friends were anxious for the performance. But the result convinced him that if this patient had been operated on earlier she probably would have recovered.

DR. IRWIN mentioned a case which occurred in the Southern Hospital, Manchester, some time ago. The fluid which came away at the operation was the thick, chocolate-coloured stuff described by Dr. Atthill, and the cyst was so brittle and friable that the trocar could not take hold of it. It all broke down, and they had to draw it out of the abdomen. A good deal of the fluid escaped into the perinæum, but they sponged it out carefully and the patient recovered.

DR. MACAN said the question whether ovarian tumours should be operated on early or late depended, he thought, upon whether the antiseptic method were used or not. Where no serious adhesions were diagnosed, he would advise that the operation should be undertaken with the aid of antiseptic surgery.

DR. HENRY KENNEDY said that, in cases of fever, they could not trust too much to temperature. He had repeatedly seen cases in which death occurred where the temperature never exceeded 100° and 101° ; and the same thing was true with reference to the pulse. The tendency was to place too much value on the thermometer. He would rather endeavour to judge by all the symptoms present. He had notes of a number of cases in which the temperature fell from 104° to 100° , and yet the patients then died. There was scarcely a disease in which he had not seen death occur while temperature was ranging through what they might call moderate degrees. With respect to vomiting, green vomiting was not necessarily connected with peritonitis at all. It was quite common in fevers. It was not a common symptom while the bile was yellow, but it was a very common symptom at the commencement of smallpox and other diseases. The black vomit was a more serious as well as a rarer symptom; and, when it occurred, the patient seldom recovered. He had seen three or four

cases of rupture of the uterus, and they were all attended with black vomit, occurring immediately afterwards.

DR. ATTHILL, in reply, asked how was the recurrence of the green vomiting to be explained? It might not necessarily be a symptom of peritonitis; but, on the other hand, this woman had no fever. She was, apparently, in perfect health at ten o'clock in the morning, and before ten at night she had green vomiting without pain or discomfort of any kind. His explanation was that either some peculiarity in the condition of the woman's constitution produced the tendency to decomposition in the walls of the cyst, or that that decomposition, after having set in, produced conditions of the system analogous to blood-poisoning.

New Cephalotribe.

DR. ATTHILL exhibited to the Society a cephalotribe sent to him by Dr. James Smith of Belfast, and read the following extracts from a letter which accompanied it:—"The following are the chief peculiarities of the cephalotribe I send you—1st. It is longer than those in use, being fifteen inches, of which the blades are ten, and the handles five—therefore better adapted for operations at or above the brim. 2nd. It possesses a double curve similar to, but smaller than, Barnes' double-curved forceps. This facilitates introduction, and renders slipping less likely when the head occupies a very anterior position above the pubes. Here the straight cephalotribe fails. 3rd. The instrument is much less formidable (though quite as efficient) than those of Hicks and Kidd, &c., and can be applied in the same manner, and with as great facility, as Barnes' forceps. 4th. When the screw is applied to its utmost limit, the space occupied by the blades is *about two inches*. 5th. The shoulders, in addition to affording greater tractile power, facilitate *rotation*. 6th. The fenestræ yield the following advantages—1. They add to the lightness of the cephalotribe. 2. As compression goes on, the head bulges through, and enables the instrument to hold with much greater tenacity. 3. For the same reason, expansion is obviated (to a great extent) in one direction, while compression is going on in another. 7th. The grooves prevent slipping also, but these might perhaps be dispensed with, as no such result need be anticipated. Moreover, in less extreme cases a child might be brought away alive, *little mutilated*, by a *grooveless tool*. Of course the probabilities are that permanent injury, resulting in death sooner or later, would be inflicted; but if it should live only to survive the rite of baptism, it would be a great relief to the consciences of a large section of the religious community. I believe, however, myself in the total extinction of foetal life in such cases, and therefore prefer the grooves; but when we have occasionally to defer to the feelings of others, I think it desirable to mention that the use of this instrument is not *altogether* incompatible with the child's life. I entertain the belief that the

compressed head (except abnormally enlarged from hydrocephalus, &c.) can be drawn through any diameter which will permit the passage of the shoulders, and the difficulties encountered in upwards of 2,000 labours (which include all the complications and operations in midwifery except the recently-imported one of gastro-elytrotomy) have strengthened me in this opinion. This cephalotribe has been used in five cases by Professor Dill and myself with the most unvarying success. In all the cases craniotomy had been performed on previous occasions—in some more than once. My instrument effected delivery in them all without having recourse to the perforator or other tool. Its utility will be best illustrated by the following case:—Instrument and foetus shown at North of Ireland Branch Meeting of British Medical Association, Jan. 17th, 1879. Child weighed at time of birth $8\frac{1}{4}$ lbs., and was extracted from a woman on whom the operation of craniotomy had been twice performed (by Dr. Dill and others). On the last occasion the exostosis on sacral promontory had progressed to such an extent as to preclude the application of Barnes' forceps in any diameter. My cephalotribe was applied by Professor Dill and myself, and delivery was accomplished, with ease and safety, in almost as many minutes as the previous confinements occupied hours. I trust it will prove of some interest to the Obstetrical Society, but particularly do I hope that it will prove of some service in the delivery of the unfortunate sufferers for whose benefits it is especially designed."

DR. MORE MADDEN said he took especial interest in this instrument, as it appeared to him to be a modification of a long compressing forceps he had exhibited here some time ago, the chief difference being that Dr. Smith's cephalotribe was somewhat shorter than his forceps, and the blades were grooved. In all other respects the two instruments appeared to him nearly identical, except that Dr. Smith's instrument was previously designed to destroy life, whilst his was designed to save life. However, the resemblances between his old forceps and Dr. Smith's new cephalotribe were, he was certain, merely accidental coincidences, and he thought Dr. Smith deserved great credit for making the cephalotribe a less formidable-looking instrument.

DR. MACAN said the instrument before them was a very good illustration of the impossibility of anybody inventing a forceps or a cephalotribe now-a-days that had not been already designed. The difference between Dr. Smith's instrument and Dr. More Madden's appeared to be that one was called a forceps and the other a cephalotribe. Why the same instrument should be employed for preserving and for destroying life he was a little at a loss to see. The present instrument, however, appeared to be an extremely handy one. His objection to the one designed by Dr. Kidd was the straight piece at the end of it.

DR. KIDD said the instrument sent by Dr. Smith was the lightest he

had seen, except that invented by Dr. Charles, of Calcutta, which it very closely resembled. He (Dr. Kidd) still maintained that the pelvic curve was a mistake, and that the transverse bars were so likewise. The cephalotribe was chiefly used when the narrow antero-posterior diameter of the pelvis rendered such instrumental means necessary. In such a form of pelvis the head did not lie in the same position that it occupied in a normal pelvis, and the instrument had to be introduced in one or other of the oblique diameters of the pelvis; and when the head was caught near the occipital protuberance it was more likely to slip out of a curved than out of a straight instrument. And after the head has been seized it is necessary to turn it, so that the compressed portion will come into the antero-posterior diameter, and so pass through the narrow portion of the brim. Sometimes the head will turn without any such assistance, but at other times it will not, and must be assisted to do so by the operator; and in either case the pelvic curve is a great disadvantage. Since he drew attention to that point all who had written on the cephalotribe had agreed that the pelvic curve should be made a great deal smaller than it used to be. The present instrument had perhaps as small a pelvic curve as any cephalotribe that had been brought forward, and, so far, came nearer to what he considered to be a perfect instrument. Still, if the head were seized with an instrument ever so little curved, and a turn were made, too much space would be occupied. With regard to the bars, they were injurious also. He had dissected a good many heads upon which cephalotribes had been used, and had never found one in which the base of the skull was fractured. It always turned so as to lie nearly flat between the blades. Dr. Kidd showed a number of casts of heads, illustrating the position of the base of the skull between the blades, and proceeded to say that many cephalotribes were made with teeth, and others with serrated ridges—the effect of that was, no doubt, to hold the head tighter, but it prevented the base of the skull from turning. The skull could not be fractured by such an instrument, and it would not get canted in the same way in which it would with a smooth instrument. The instrument exhibited might hold more tightly than a smooth-bladed cephalotribe would do, but he did not think that an advantage. If there was a difficulty in extracting the head, instead of using force it was better to adopt the plan of making repeated crushings—that is, if the object could not be effected at once it was better to apply the instrument a second time, and seize the head in another direction. Pajot had done so as often as five or six times. He recommended that the head should be crushed, and that the expulsion of it should be left to nature. Dr. Kidd thought that was a mistake, and that instead of converting the soft parts of the mother into a means of compressing the head, it was better to crush the head repeatedly, until they got it to be so moulded by that process that it could be extracted easily. He, therefore, adhered to his

opinion in favour of smooth straight blades. As to allowing the head to bulge through the fenestræ, it was no advantage either. Many writers and speakers had asserted that when a head was compressed in one direction it expanded in another, but that was not the case, as the casts showed.

DR. M'CLINTOCK.—Did you use the perforator always?

DR. KIDD.—I did always. I think this instrument has an advantage in being lighter than mine. Dr. Charles's is a lighter one still.

DR. ATTHILL said it had been his misfortune to be obliged to use the cephalotribe on several occasions. One objection to Dr. Kidd's instrument was its weight, and another was its exceedingly short handle, which made it difficult to manipulate. He also thought Dr. Kidd's instrument more likely to slip than the one now before them would be. In the majority of cases that he had operated on, the head had been tilted very far forward; he had always used the straight instrument designed by Dr. Kidd, and had found great difficulty in preventing it from slipping backwards towards the sacrum, and, in order to remedy this, had had to pull it out and re-adjust it. Of course he had always succeeded in delivering with it, but still he had experienced that difficulty. He did not think it would occur in the instrument before them, which would, probably, be more easy to apply and less liable to slip, besides being lighter, and affording a better grip for the hand. But he must say his impression was that in any very difficult case, with a very narrow pelvis, Dr. Kidd's, or any other straight instrument, would be the more efficient. The ribbing was, in his opinion, a mistake. The fenestræ, he thought, not a matter of importance. He considered Dr. Smith's a nice instrument, and calculated, in the majority of cases, to be more easily used than Dr. Kidd's.

DR. M'CLINTOCK said that one novel point about Dr. Smith's instrument was that it appeared to be used without previous perforation. It was also said that it could be used with a hope of extracting a living child. If the instrument were to be used as a cephalotribe, they might extinguish all hope of saving the child. If it were used as a forceps, the matter was different. He concurred with Pajot in regarding the cephalotribe, not as an instrument for extraction, but for reducing the head to such a state of pulp that a very small amount of effort would be required for its removal. Viewing it in that light, the transverse furrows were decidedly objectionable. He agreed with Dr. Kidd that the double curve was rather a mistake. As Dr. Kidd had said, the object was to compress the head, and bring the most compressed part towards the most narrow part of the pelvis; but if the head was caught laterally, it was quite clear that the most compressed part would not coincide with the narrowest part of the pelvis. A necessity for turning it in the pelvic curve was an inconvenience, and an objection to the use of the instrument. The same reasoning applied to the use of the long double-curved forceps.

Brow Presentation.

DR. KIDD said he desired to say a word on the cast now before them. Brow presentation is very rare, and when it occurred in a narrow pelvis the complication was very unpleasant. The cast represented the child of a third pregnancy. In the mother's first labour she was delivered with a crotchet. There was some doubt as to how her second labour resulted. She stated that she was in the Richmond Lunatic Asylum for some time, and that she was delivered of a living child there; but her friends seemed to be not quite certain as to the facts. If it were a living child it must have been premature. She stated that it died a short time after its birth. When I saw her the head was entering the brim. Dr. Roe had seen her previously, and made out that it was a brow presentation. The nose, eyes, and even mouth could be felt. They tried to push up the brow and convert it into a vertex presentation, but failed. Dr. Roe tried the straight forceps, but it did not succeed—not from slipping, but because the head would not come through. He (Dr. Kidd) tried Barnes' forceps, and used as much pressure as he thought was at all justifiable, especially as he thought the antero-posterior diameter of the pelvis was under three and a quarter inches. The head was pretty well engaged in the pelvis, the membranes had ruptured two days before, and the uterus was pretty well moulded to the child, and to turn it would have been attended with a great deal of danger. She had one formidable symptom, which was that the colon stood out along the right side of the uterus, and was very tympanitic. He had often seen that symptom in prolonged cases of labour, and had known it to be followed by bad results. It was probably an indication of commencing peritonitis, and of paralysis of the muscular fibre of the intestines; and it was a symptom that made him very unwilling to subject the patient to any violence that could be avoided. He perforated the head at the frontal bone, and then applied the cephalotribe. One blade of it lay on the face, the point entering the mouth, and the other caught well down on the occiput. After the head had been compressed and the cerebral matter had poured out, he attempted to deliver, but the child did not come. He then turned the instrument, and it went round with a jerk; and as soon as he had turned the compressed portion of the head into the narrow portion of the pelvis, it came out as easily as a glove off the hand, and he had no further difficulty in the case. The woman recovered without a bad symptom.

The Society then adjourned.

TRANSACTIONS OF THE ULSTER MEDICAL SOCIETY.

SESSION 1878-79.

THE ULSTER
MEDICAL SOCIETY FOR
OBSERVATION

President—A. HARKIN, M.D., J.P.

Hon. Secretary—WILLIAM WHITLA, M.D.

Third Meeting, December 17, 1878.

A. HARKIN, M.D., President, in the Chair.

Adjourned Discussion on the Milk-feeding of Infants at Nurse.

DR. WHITLA said: Mr. President and Gentlemen,—My name appears upon the paper this evening as re-opening this discussion, but the announcement is merely a formal one, as it would be unpardonable for me to detain the Society with remarks upon a subject which is simply a matter of experience. In listening to the remarks at last meeting I was struck with their apparent diversity, and I believe this arises simply from the angle at which each one looks at the matter, not from any real difference of opinion. All are agreed that a healthy mother's milk as food for her offspring cannot be improved upon—all, I believe, will agree that leaving out the moral objections to wet-nursing, and the objections of expense and expediency, that it is the best substitute, and it is only in cases where these two supplies fail us that we have now to consider the best course to pursue. For obvious reasons the milk of the cow is the pabulum nearly universally selected, and the question of dilution of this fluid is one which it is an utter impossibility to settle by any fixed rule. Our President strongly objects to the addition of water on the ground mainly that it has got enough already before it reaches the nursery, and I have no doubt that generally he is correct. My own limited experience in the Children's Hospital has thoroughly satisfied me that children are starved to death upon milk and water in Belfast. If the question is put to me—would I give milk from the cow to a young infant, I would answer that this would entirely depend upon the feeding of the animal. Not long since I ordered the pure milk of a fine cow brought from the country by a gentleman in Belfast very anxious to procure pure nourishment for his infant which was four weeks old. The result was near being disastrous, the motions became as firm as cheese, and the child began to sink from starvation of a worse kind than that so eloquently painted by our President. I was then forced to do what should have been my first duty—to inquire and see the cow's food, which

I found was the best meal and potatoes, and also to have her daily produce measured, which was only nine quarts. The same cow had a week previously given nearly twenty quarts when fed upon boiled turnips.

This, gentlemen, is no exceptional case—it is the rule, and this fluctuation is what physiology would lead us to expect. Some affirm that though the *quantity* of milk produced by a cow changes wonderfully on altering her rations, still, they say, the *quality* remains steady. This I do not at all believe, as I have myself in the country and within the last month in Belfast proved. It is, however, a fact that the character of the milk will also depend upon the cow *to an extent* totally independent of feeding; and I do not profess to be a cow-fancier, but there is to my mind no cow like a thorough-bred Devonshire for raising strong and vigorous infants. Dr. M'Connell stated his experience in the Union, which is of great value, and I believe his position about this question of dilution is the only truly safe one—that is, “to adopt no rule, but judge solely by the quality of the milk when you do not know the food which the animal consumes,” and dilute or not as the case requires.

I believe the practice of diluting the weak milk of cows forced by soft and watery foods, as is the rule in Belfast, has led to many fatal results; and we owe to our President a great debt for bringing the matter so ably and prominently before the Society. One word about the temperature of the milk. This I believe to be of the same importance as the temperature of the air breathed by the infant. Provision is made for variations of nearly 100°, and I think it is a matter of no practical moment whether it be 50° or 100° with a healthy child.

In conclusion, allow me to refer to a class of cases where cow's milk—no matter how pure or well selected—fails to nourish, and we are driven to a further extremity. Every one has met such. It is in these cases that attempts to utilise the teachings of the science of chemistry have resulted, I believe, always in increasing the number of pages in the Registrar-General's Returns; and if there be an example of the truth of Pope's line, “A little learning is a dangerous thing,” surely it is in the feeding of infants upon purely chemical principles.

What are we to do in such a case? My practice has always been, where I could prevail upon the hearty co-operation of the parent, to resort to beef. Good cow's muscle, under-cooked, sucked to rags by the infant, never fails to sustain life where life is possible, and within these last two years I recommend it to all children. At present I am trying a modification of this practice suggested by Dr. Beck, senior. He recommends strong beef-tea, half a pint, one well-beaten-up egg, and half a pint of milk. As far as I can see it is an invaluable plan where cow's milk fails.

Dr. Core has asked for some information about Nestle's food. It is

the only artificial food of which I have any experience, and I have seen splendid results from it. The great objection to its use is its expense. I have seen a strong infant eat twelve shillings worth per week, and this puts it beyond the reach of the poor.

One speaker referred to the infant mortality from a national point of view, and I feel I would not be fulfilling my duty as a physician, and as an humble unit in this vast empire, without seizing this opportunity of stating that the wholesale slaughter-houses in our midst should be under Government supervision. I refer to the system of nursing the children of mill-workers, and the nursing out of illegitimate infants. The Government has started a cumbrous and expensive machinery for watching the exact number of minutes per week each able bodied operative will be allowed to work, lest the race should deteriorate, while the infants of these men are half-starved, and dwarfed to a degree which hardly anyone could believe without visiting the extern wards of the children's hospitals. And I often feel, while looking at their pinched and weird faces, that their cry for Government protection should be heard before that of lunatics, monomaniacs, and others.

DR. JOHN MOORE said he should like to know the exact amount of milk considered necessary for an infant. He detailed a case showing the remarkable change which supervened upon the admission into gaol of a woman and her child, after she had given it her undivided attention. He long held that the State should interfere, and agreed with the Secretary that the question was a momentous one. He detailed another case where the child was buried alive, discovered, and taken to prison with the mother, and afterwards left there—as fine a specimen of humanity as he had ever seen. He advocated that women in the last months of pregnancy, and nursing mothers, should be excluded from public work; and he argued that it would be cheaper for the State to support both. He also thought that the food of the cow was of vital importance in the rearing of infants upon the bottle.

DR. HARKIN.—Gentlemen, the discussion evoked by the nature of my Introductory Address has been the source of much satisfaction to me, having called forth an amount of practical statements and observations of a most valuable character, based, as they were, upon the personal experience of the individual speakers.

I cannot regret that the debate should have taken a wider range than I anticipated, or that it should have assumed the form of a general disquisition upon infantile nutrition and the varied aliments and modes of feeding practised and recommended in this country; for, incidentally, much information has been elicited, and further proof has been given, if any were required, that the medical men whom I have the honour of addressing are not obnoxious to the charge of being merely the slaves of routine, when a subject of such vital importance is before them. My friend,

Dr. Wales, who initiated the debate, among other sound remarks, stated as his opinion "that milk, as ordinarily sold, requires no dilution; but that he would not venture on ordering undiluted milk to a new-born baby." In both these propositions he and I agree, for I should not think of recommending milk alone until at least two weeks had passed over, and until the integrity of the infant's digestive organs had been fully established. He says, further, "I cannot think it wrong to reduce the proportion of casein and salts in the cow's milk to those existing in human milk, by the addition of water, and of lime-water also, if disposed to acidity, with the object of preventing the formation of coagula, which often give rise to cholera infantum." While, however, I accept his endorsement of my proposal, I cannot do so for the reasons assigned by him, as I have had, too often, sad experience of the persistence of infantile diarrhoea, until every, even the smallest, portion of milk was eliminated from the dietary of the suffering child. In this statement the high authority of the late Dr. Graves will sustain me; for in his "*Clinical Medicine*," edited by Neligan, Vol. II., page 583, he says:—"It is incredible how small a proportion of milk, even in the most diluted state, will keep up this disease, acting like a species of poison on the intestinal mucous surface. You know that animal poisons, such as the variolous or vaccine virus, will affect the system, even when applied in a state of extreme dilution, and you can, therefore, conceive that a small portion of milk will operate in this manner." I wish, however, to corroborate, to the fullest extent, all the commendation which Dr. Wales has given to the use of the condensed Swiss milk. I have had, in many instances, the greatest satisfaction from its use. In one remarkable case, wherein the breast milk of as many as seven hired nurses disagreed, and where the child's digestive organs were quite intolerant of cow's milk, from the first dose of the Swiss milk all trouble ceased—the child slept calmly, content spread over its features, the alvine discharges, previously most offensive, became natural—a change which I attributed to the elimination of the casein from the condensed milk, during the process of preparation. I saw this infant some two years after, and she had grown into a healthy, well-proportioned child, and had just passed unharmed through a severe attack of measles. So much for Dr. Pavy's theory. Professor Dill, in a statement replete with information, among other matters, stated that he would dilute the milk with a little water, and that as the mother's milk grows stronger as the child grows older, he would, following the example of nature, decrease the ratio of dilution as the child advances. He gives the preference to asses' milk. We must remember, however, that, according to Playfair, asses' milk, although containing as much sugar as cows' milk, is too poor in casein and butter for the feeding of infants. Dr. Dill announces his strong preference for wet-nursing in contradistinction to bottle-feeding, and in this sentiment I quite concur.

Dr. Dempsey would dilute with one-third of water, with the addition of a little sugar of milk, and gives very judicious advice as to the care required in bottle-feeding. He attributes the dyspepsia and marasmus of hand-fed children more to the carelessness of those who feed them than to the dilution of the milk itself. I cannot assent to this theory; and Ancell on Tuberculosis, in a chapter on Diet, page 703, is also of a different opinion. He says, referring to children:—"I am practically convinced that habitual excessive dilution is very injurious in tubercular habits. I believe it assists to diminish the vitality of the blood and the number of the red corpuscles, to render the blastema and liquor sanguinis, less nutritive, and to deteriorate the cells and weaken the fibres." He gives also the case of an infant, three months old, in a very advanced stage of marasmus, which completely recovered on goat's milk. "It had nothing but the breast of one and the same goat for five months, and is now a perfectly healthy child." Thus we find that excessive dilution on one hand deteriorates the blood, and that pure unmixed milk is the remedy for marasmus. Dr. Dempsey condemns very strongly the too early administration of farinaceous food, but says that if any other than milk food is suitable for early infancy, the only one he can recommend is that prepared by Liebig. We must not forget, however, that, although Dr. Playfair joins in the recommendation, its use has been condemned by the Academy of Medicine in Paris, after being discussed at several sittings. We cannot, therefore, acquiesce in its suitability until the difference of opinion which exists be settled by an independent examination, by some medical tribunal in this country. Dr. Walton Browne would dilute with barley water one-third, up to the ninth month. Dr. Riddell's experience quite agreed with that of the President. Dr. Lindsay very properly insists that, when children are bottle-fed, the medical man should see that the milk be pure, and given by cattle themselves healthy; and Ancell, page 701, appears of the same opinion. He says:—"I have seen an infant deprived of its mother's milk, because she was of a tuberculous habit, and fed upon the milk of a cow which died of consumption. The child died of tubercles in almost all of the internal organs. The frequency of this disease in cows is a notorious fact. Dr. Workman showed by statistics that the mortality in foundling Continental hospitals is greater in bottle-fed than in nurse-fed infants. Dr. Core believed that a great evil arose from people trying to improve on the proportion of nutriment given by the Creator. He strongly disapproved of wet-nursing of children. Milk, as sold in Belfast, did not require any further dilution. Dr. Esler disapproved of a practice common among the older nurses of feeding the infant before putting it to the breast, and instanced the case of the aboriginal tribes of Victoria, where the infant is put at once to the mother's breast after the birth is completed. He believed that multitudes of our infant population are starved to death by over-feeding. He would

recommend the addition of one-third water till the sixth month of infantile life. He approved of Nestle's food. It is, no doubt, true that, in many instances, children are over-fed before the free secretion of milk appears; but the opposite evil is, in my experience, of more frequent occurrence. Mothers and nurses imagine that the turgescence of the breasts is a proof of the presence of milk, and many an unfortunate baby is compelled to tug and strain at a breast quite innocent of lacteal secretion. The infant starving, sometimes to the very verge of inanition, and often only saved by (I speak of my own experience) the free administration of wine-whey and milk. I do not consider the case of the Indian woman who, as a rule, is able to suckle her child for three or four years, at all analogous to that of the European mother, subject to all the depressing influences of modern civilisation. Dr. Whitla, in a well-reasoned article, among other remarks, states that his experience in the Children's Hospital has thoroughly satisfied him that children are starved to death upon milk and water in Belfast; and if he were asked the question would he give the milk from the cow to a young infant, he would answer—this would entirely depend on the feeding of the animal; and he gives a typical case in which the pure milk of a cow disagreed with a baby, the animal having been fed upon stimulating farinaceous food—food fitted only, in my mind, for a human being. the animal being denied the natural food of a graminivorous creature. I can, however, point to a child, now six months old, fed entirely, for many months past, on the milk of a Kerry cow, whose sole food is to be found in the grass of the field, and no better example of a rosy, thriving child could be found; and besides to many others in town and country, who have not had any other nourishment but the undiluted milk direct from the cow—the nature of the food, as the Doctor suggests, making all the difference possible in the nature and composition of the milk. Dr. M'Connell diluted, but in regulated proportion to the individual case, not on any preconceived idea, healthy and unhealthy children requiring special provisions. He could not tell the proportion of water in the milk supplied to the workhouse, as he did not believe in the existence of any infallible test, and was guided chiefly by its taste and appearance. It is, however, a matter of vital importance to look after the composition of the milk delivered to workhouses, as it enters into the diet of children, and old persons, too, in workhouses as a most important element. Mr. Wanklyn found that of fifty-seven samples of workhouse milk only three were up to the terms of the contract. Most of them had been deprived of a large portion of the cream and diluted with water to a serious extent. (See *British Medical Journal*, January 27, 1872.) Dr. John Moore, from his experience, placed great dependence upon the undivided attention of the mother, and thought the Legislature should interfere to prevent the too speedy return of a young mother to work in a factory. The President agreed in both

statements, and instanced the case of the town of Coventry, where it was feared that in consequence of the mills being closed in which the women worked at ribbon-making, caused by a change in fashion some years since, infant mortality would increase. It was found, on the contrary, that it had a marked decrease, proving that the maternal care given by mothers at home, through necessity, more than compensated for the better fare provided when the mothers were in employment and earning high wages. Dr. Fagan's experience coincided with that of Dr. Esler, and believed judicious starving useful in many cases. The President was of opinion that the children of the poor were not alone injudiciously, but also often over-fed—even in their infancy partaking of the farinaceous and animal diet provided for their parents. The children of the well-to-do suffered from the opposite error, that of having to live upon milk diluted and barren of every nutritious material—the child, in many instances, seldom tasting milk except in tea or some other potation.

Fourth Meeting, January 7, 1879.

A. HARKIN, M.D., President, in the Chair.

Notes on Croton Chloral and Nitrite of Amyl. By R. RIDDELL, M.D., &c.

MR. PRESIDENT AND GENTLEMEN,—I thought it desirable to bring before the notice of this Society a few facts and experiences coming under my own notice concerning the medicinal properties of the so-called "Croton Chloral" and "Nitrite of Amyl." The literature of these drugs is not very extensive, but still sufficiently so to enable one to fill up the space of a long paper with notes and cases already reported. I have chosen another course, and prefer being brief and merely relating cases which have been under my own care, and which I have watched attentively. Neither can I enter into any discussion concerning their chemistry, nor concerning the physiological reasons of their peculiar actions on the *diseased* frame. I emphasise the word "diseased" for the reason that, as far as I have noticed, their actions in health and disease are quite different.

My first experience of croton chloral, as it is wrongly called, was in the person of a near relative, who for years had suffered from severe attacks of headache—so severe that at the times of attack she was quite prostrated. The constant description she gave to the medical men she consulted was that of feeling as if an iron band were round her head and crushing it. These attacks were recognised by all as neurosal in origin, and for years it was attacked by all the "great guns" of the Pharmacopœia until its resources were exhausted—the bromides were tried up

to so-called "heroic" doses ; chloride of ammonium, iron, arsenic, hydrobromic acid, sedatives and stimulants ; quinine, in small or large doses, only made it worse—all of no effect for good. I was reading some remarks in *The Practitioner* concerning croton chloral, and it seemed to me that it might do good in this case. I suggested it to her, and she consented to take it, but with no idea or hope of any relief, for she had suffered from these attacks for more than twelve years. I began with 5-grain doses twice daily and 10 grains going to bed, dissolved in spirits of wine and glycerine, with a little acid and syrup of orange to cover the flavour. The good effect of the drug was seen at once ; the attacks came at longer intervals, and were less severe, then ceased altogether ; and now for upwards of seven months she has had only two attacks—one at a time of great mental distress, and one when she stopped taking the drug, about two months since, when she resumed taking it, and has not had an attack since. She now takes 5 grains per diem (generally at night), is hearty and well, and has a good appetite. Her age, I may say, is sixty-three. The 5-grain doses only caused a slight drowsiness, but the 10 grains at night, by relieving all pain, gave her a good night's rest—in fact, she now enjoys better health than she has done for years.

Since that time I have used it largely—sometimes failing, sometimes relieving—till, by keeping an account of all my cases, it began to dawn on me which were benefited by the drug. Since then the number of cases relieved (some permanently) has increased. These cases are—headache in females arising from mental distress ; those cases of headache so frequent at the menopause—in fact, all those called neuralgic, except a few arising from internal mischief, are benefited, and, in many instances, cured. In that distressing species of neuralgia called tic-douloureux I have found it in many cases acting like a charm. Of course I do not include any arising from cranial or intercranial causes. I have tried it in neuralgia of the ovaries, but no good resulted. In insomnia it is not so reliable as the hydrate, but in some cases where the loss of, or inability to, sleep is accompanied by a weak or fatty heart, it is to be preferred, as it has no weakening effect on the central organ of the circulation. In one case of delirium tremens, where the circulation was very feeble, the combination of croton chloral with digitalis had a wonderful effect, and it seemed as if the drugs could be given together in much smaller doses, to produce the same results, than singly. In this I pushed it from 10 to 30 grains every three hours, with drachm and 2-drachm doses of the infusion of digitalis. In pain arising from caries of teeth I have found it useless in most cases, and in all inferior to Richardson's "tr. gelsemini;" but in one case of a nervous young lady, by giving her two 10-grain doses I was able to extract a tooth next to painlessly, to her great satisfaction. You will notice in all these cases it is in affections of those parts supplied by the fifth pair of nerves that it is of most use ; but, to be of service, you must

give the drug in far larger doses than prescribed in the Pharmacopœia—for adults, 5 grains three or four times daily, gradually increasing if required; if stimulants are wanted, dissolve it in rectified spirit; if not, dissolve it in glycerine. In all cases complicated with hæmorrhoids, give glycerine. If anæmia exists, combine it with iron, or, which I believe better, arsenic; then gradually lessen the chloral. In all cases I have found it better to give it in solution than in powder or pill.

Since last meeting, when it was intended that I should have read these notes, some cases of great interest have come under my notice, in conjunction with my observation on the fact that croton chloral exerts most influence on painful affections of the superficial nerves of the face and its adjuncts. On the 18th December I was consulted by J. A., aged twenty, a striker in a smithy, for “something in his eye.” I made careful examination, and could find nothing, although there was intense photophobia—tears streaming through the “clenched” eyelids. I dropped in atropia and counter-irritated his temples, which I have often seen doing good, but to no purpose. Another examination discovered a minute abrasion of the conjunctiva. I dropped in oil and atropia again, rubbing the extract of belladonna over the eyebrows; no effect. It struck me to try the chloral, seeing its power over these nerves, and it was most wonderful. I gave him 10 grains at once, and repeated that dose in an hour, when considerable relief was experienced. I repeated the 10 grains in two hours more and the pain was entirely gone, though for precaution’s sake I gave him 5 grains every three hours for a day or two.

J. F., brush-maker, consulted me on the 21st for facial carbuncle. You all know the terrible pain suffered in that disease. To control that pain and give sleep I gave opium, Indian hemp, and the hydrate of chloral. Opium gave relief at first, then failed on account of stomach derangement. The other two were useless, or nearly so. Following out the same line of thought as in the former case, I gave the croton in 10-grain doses. The effect was simply marvellous. Giving 10 grains every three hours, the disease went through its different stages, and very seldom would the patient have known of anything the matter from the sense of feeling.

The last case was a lady who has been under my care a long time, and who for months suffered from neuralgia, coming on at 12 o’clock, noon. In fact, she said (a little bow-stretching, you will say) “I can set my watch by the attack.” Considering the periodicity, I gave arsenic, nuxvomica, iron, and a whole array of tonics. I then (since last meeting) gave the chloral; and after two days she has had only one attack, and that a mild one.

I do not vaunt croton chloral as a specific—I do not believe in the term except in one instance—I do not compare it with, or fight its battle against, any other drug—no physician should have a “favourite” drug or prescrip-

tion—but in my hands it has been useful in relieving pain, and I use it as I would any other instrument for good.

I have only one or two facts concerning nitrite of amyl to bring before you. The first case is that of a young married lady with menorrhagia, who, at the times of the catamenia, suffered from terrible headaches—always relieved, however, by hot spirits and water. She herself and her friends were desirous of some other remedial means than spirits. I came to the conclusion that the pain was anæmic in origin; and the nitrite of amyl possessing the dual power of increasing the heart's action and dilating the intercranial vessels, I thought it would do good; so I mixed the amyl with equal parts of eau-de-Cologne, and ordered her to use 5 or 6 drops of the mixture dropped on a handkerchief and inhaled by deep inhalations. It succeeded perfectly, and ever since she has never been without that combination—all other remedies used to put an end to that infliction having failed.

One night I was called out to see a case—a woman aged about fifty-four—"heart disease" the friends called it. When I arrived I saw at once it was a case of asthma. She was in a terrible paroxysm; leaning forward on her elbows, with flushed face and dilating nostrils, she was gasping for breath. I thought of the amyl, and fortunately had some with me. I poured out 5 drops on my hand, rubbing them over the palm, and held my hand to her mouth; when she had inhaled 10 drops she could breathe naturally. A course of arsenic effectually cured her.

J. L., a clog-maker, consulted me the day before Christmas, 1877. He suffered from asthma, and the attacks were so frequent that he was off work more than half time, and the paroxysms were severe. In every case the amyl checked it at once; and now, nearly twelve months having passed, he informed me yesterday that he had only lost one week altogether, and that was through exposure to cold. I may say arsenic was the drug I gave him during the intervals.

I have had abundant opportunities of trying it in sea-sickness, but only in a few instances it did good. But in that vile sickness one is thankful for small mercies. In one of my voyages one of the passengers had brought some chloroform on board for toothache. I was summoned in haste to her cabin, and found her quite insensible, pulse not perceptible, and evidently dying. Fortunately, though the nitrite was not in the ship's surgery, I had brought some to experiment with. After inhaling a few drops the heart's action was quickened and strengthened. Three or four times we had to repeat the inhalations before she was out of danger. It is so antagonistic to chloroform that I am of the opinion that it should be at hand at every operation in which chloroform is given. I throw out the hint for any of the surgical staff of the hospitals who may be present. In opium-poisoning I have derived no benefit from it—I suppose for the reason that that poison kills by stopping the respiration

first. In fainting attacks from weakness of the heart, or brought on by witnessing an operation, or a cut, it is of great service. It has often an equally good effect in spasmodic and cardiac asthma, though its action must be different in the two diseases. In the one it must relieve by lessening the spasm of the air-vessels, in the other by increasing the power of the heart. How it does this I do not know; I am only glad to know that it is a fact.

In conclusion, I have brought before your notice what I believe to be two very valuable medicines, exercising relieving and curative effects on two distressing maladies—neuralgia and asthma, also acting as an antagonist to chloroform. If I can bring out of the more extended experiences of my *confrères* confirmation of my belief, my object shall have been gained.

DR. CORE said he had not used nitrite of amyl, and would hesitate to employ it while there still existed such grave differences of opinion about its dose and action. Upon the usefulness of croton chloral he would agree with Dr. Riddell if the fifth nerve only was affected.

DR. ESLER thought with Dr. Core that the croton chloral was most useful and efficacious where the fifth nerve was in question. He had not used the amyl; he knew of so many hair-breadth escapes—and these acted to him as a beacon—he would wait for further light before employing it.

DR. WALES had used croton chloral a good deal; it was a favourite remedy with him in pain-relieving, but he never used it in the doses mentioned by Dr. Riddell. He would not go beyond 15 grains every two hours. He generally gave from 5 to 15 grains every twenty minutes, till three doses were taken, by which time he usually found the suffering diminish rapidly. As for the amyl, he never used it himself. He knew a medical friend who injected 15 minims hypodermically; the patient, however, died, but he did not believe the amyl had been the means; but it was an open question what effect such a large dose would have so quickly introduced into the circulation.

DR. RIDDELL said he mixed the nitrite of amyl with eau-de-Cologne, and allowed it to evaporate upon the palm of the hand while the patient inhaled its vapour. He would recommend that the croton chloral be always tested for starch by iodine, as he often found it so adulterated.

*Remarks upon DR. WHITLA'S Specimens of Trichorexis Nodosa, exhibited at last meeting.**

DR. CORE had read the discussion, or rather series of letters, in *The Lancet* with interest, and he thought that three different diseases were there referred to—one, the Columbian, then a parasitic, and, thirdly, the

* See Dr. Whitla's paper on "Trichorexis Nodosa" in the number of this Journal for February, 1879, page 104.

one demonstrated by the Hon. Secretary. The subject, he thought, was one of great interest, especially as to its wide distribution. He did not believe the specimens were or could be mistaken for parasitic disease.

DR. WORKMAN, who had examined very many specimens with Dr. Whitla, could corroborate his statements in every particular. The cause was obscure, but he did not believe in the parasitic theory. Many of the hairs he noticed were split longitudinally, but only at the ends.

DR. ANDERSON thought that the pathology of the condition was simply a change of nutrition in the root sheath, and he proceeded to support this view by a reference to the growth of the nails, likening the nodes to the white specks seen in these appendages, and to the ridges and irregularities seen in cases of recovery after fevers, &c.

DR. RIDDELL did not see that the subject was one of much interest, or that there was much mystery about it. The hairs seemed broken simply from combing, twisting, and pulling. He had not noticed any hairs like those spoken of in the natives of Australia, and he consequently thought that it must be caused by the use of the comb. He thought that simple bending and brushing might do all that was described—he saw no reason why it should not.

DR. ESLER thought that Dr. Whitla had clearly demonstrated to him that this was no mere mechanical breakage, and, after examining the specimens upon the table, he thought it impossible to conceive this, nor did he believe it was parasitic.

DR. WALES said the subject was new to him, and he had looked at the microscopic specimens with care and they surprised him. He thought the distribution of the condition a most important point; it proved that it could not be regarded as a disease. He had taken some hairs from the beards of the patients exhibited, and he would examine them with interest.

DR. WHITLA said he had at first taken every pains to satisfy himself about the mechanical theory. He marked hairs upon the margins of his own face, and found that twisting, bending, and pulling to an extent far beyond what would be applied ordinarily, produced not the slightest effect. The favourite sites of the affected hairs were not those subjected to friction. He had tried the effects of torsion and bending upon wet hairs, warmed hairs, and very dry ones, without any result. Those who believed in this as the cause always supposed that in order to break, the hairs must be previously diseased—morbidly dry. He was satisfied that three different conditions were described, but was inclined to think that the presence of spores was not the cause in those cases observed in the nodes, for it was clear that the characteristic breaks could be caused independent of them. Several who had written upon the subject—and eminent authorities upon skin affections also—had spoken of seeing one, two, or three cases. His experience of its almost universal

distribution was his apology for bringing the matter before the Society ; but he would say, to be always found they must often be most carefully and diligently hunted for in some beards.

Fifth Meeting, January 25, 1879.

A. HARKIN, M.D., President, in the Chair.

Injection of Hot Water in Post Partum Hæmorrhage. By R. ESLEB, M.D.

MR. PRESIDENT,—I bring this subject before the Society—1st, because of its great importance ; 2nd, because it is a new method of treating a troublesome complication of labour ; and 3rd, because I was amongst the first to try it in this country. The history of its introduction is given in a letter from Dr. Whitwell, of San Francisco, to Dr. Atthill, of Dublin, and is briefly as follows :—

In 1874–5 in the Women's Hospital, State of New York, hot water was injected into the uterus of a patient, from whom a sarcoma had been removed, with good effect.

In August, 1875, Dr. Whitwell tried hot water injections in Breslau, and subsequently in Prague, with the best results, always causing quick and firm contraction of the womb.

Dr. Atthill, of Dublin, saw the account of these cases in an American journal ; and in *The Lancet* of January 5th, 1878, published his first two cases. I had just read Dr. Atthill's article in the week after publication, when I was sent for to attend Mrs. B., a stout woman of thirty, in her third confinement. Her first child was born two and a-half years before, after which there was extensive flooding. Twelve months after, her second child was born, when I saw her for the first time ; there was detained placenta, which had to be removed ; flooding supervened, which was treated with cold applications ; the loss of blood was so great that recovery was considerably retarded. Her third confinement took place on the 12th January last year ; the labour was normal ; the pains were regular ; but after the expulsion of the child and removal of the placenta, the uterus ceased to contract, the loss of blood was again very great, and was beginning to tell on her pulse ; she became faint and blanched. From her former experience she greatly feared the shock from the application of cold. I immediately procured a Higginson's syringe, passed the vaginal tube well into the uterus, and smartly injected half a basin-full of water at about 112°. Almost instantly the uterus contracted firmly, the hæmorrhage ceased, the pulse improved, and the patient expressed herself as feeling most comfortable, and the anxious countenance gave place to a placid calm. After the expulsion of some clots all went on well.

During last Summer I met with two cases demanding more than the usual application of cold—one of them where a midwife thought the

patient dying. I need not trouble you with details; the same means used brought about the desired results; and, from what I have seen, I have no doubt that hot water is one of the best agents we possess to cause safe and speedy contraction of the uterus. In each case I have been impressed with the rapidity with which the exhausted patient rallied, and with the expression of a feeling of comfort imparted by hot as compared with cold applications.

DR. WALES had no experience whatever of the injection of hot water, but if a case occurred in his practice he would have no objection to try it. He spoke very favourably of the injection of ergotin subcutaneously.

DR. LINDSAY thought that the case was very graphically painted by Dr. Esler, but he would be very slow to endorse the general use of hæmostatic remedies. In a very large practice of ten years he only met with four cases which he would call cases of *post partum* hæmorrhage, and he would like to hear Dr. Esler define what he meant by *post partum* hæmorrhage. He thought that many cases occurring with young practitioners were called cases of *post partum* hæmorrhage which were not.

DR. D. JOHNSTONE had no experience of the hot water treatment, but any remedy which fulfilled the indications stated by Dr. Esler would be a welcome one. He would not be inclined to use water at a temperature of 110°; ice at all times responded to his call. He also found that the hand put in cold water and then introduced into the uterus and maintained there for a short time, had a very decidedly beneficial effect. He asked for an explanation of the way in which hot water was supposed to act.

DR. O'MALLEY had a good many cases, but he never had to resort to the hot water or to iron; his cases all yielded to cold water, applied externally, with the hypodermic injection of ether.

DR. BROWNE always used ice in bad cases, and never lost a patient. He commented upon Dr. O'Malley's case of ether as being the first in the North of Ireland.

DR. WORKMAN explained how the hot water caused the stoppage of hæmorrhage, and detailed the effects which he had observed by the application of hot water to frogs; he found it tetanised them by a powerful action upon the entire nervous system.

DR. JOHN MOORE thought that there may be difficulties in wandering away from old paths. One thing which, however, would speak loudly for this new treatment was the universality of the remedy; ice might be difficult to find, but hot water was everywhere; he never used it however. The mortality after delivery was much higher than most men imagined; he had satisfied himself that 1 in 150 was about the true figure. He detailed a case where he saved a life by pressure up on the aorta, and he reviewed other methods of treatment.

DR. CLEMENTS had a large experience as Registrar, and he would state that the mortality was not nearly so high as Dr. Moore stated; he only

remembered one case being registered as caused by labour in some six hundred cases.

DR. HARKIN had not tried the hot water—he was perhaps too fond of sticking to the old ways. As regarded other methods of treatment, he reviewed them in detail, and said that when everything failed, “he put a cork in the bottle.” He seized the uterus with one hand firmly, introduced the other into the vagina, and so held it that it was impossible for one drop to escape, and in some cases he had to introduce a sponge in vinegar. If he found after a time that the pulse did not rise, and that on removing his hand a tendency to return occurred, he then injected ergotin, and it always succeeded. He detailed a case where he injected ether two and a-half years ago—the patient, however, died. He would hesitate to use hot water.

DOUBLE HEARING—AUTOPHONY AND TINNITUS AURIUM.

DR. SAMUEL SEXTON (*Transactions of the American Otological Society*, 1878) seeks to elucidate some of the hitherto unexplained problems of audition, especially those connected with double hearing, autophony, tinnitus aurium, hearing better in a noise and the contrary. He believes that these conditions most frequently depend on derangement of the conductive apparatus rather than on pathological conditions of the inner ear, as is usually maintained. The various anomalies of audition, double hearing, &c., are of much greater frequency than is generally supposed. On examination it is found that some patients can hear their voice in two distinct ways—first by the medium of the external air and external meatus after the sound has issued from the mouth; second, by conduction from the vocal cords directly through the intervening tissues. Dr. Sexton believes that this occurs in cases where the articular surfaces of the malleo-incudal joint are separated as a result of disease. When this occurs in one or both ears much confusion is apt to result. Musical persons sometimes complain of hearing musical notes falsely. Most authorities explain this by referring the abnormality to the auditory nerve itself. Dr. Sexton, however, believes it is due to imperfect conduction of the sound by derangements of the conductive apparatus. Tinnitus aurium he explains as due to the sound waves of circulation in the neighbourhood of the ear—but not in the labyrinth—which ordinarily are not heard, but when the malleo-incudal joint is separated, these vibrations of sound reach the acoustic nerve through the incus and stapes. Dr. Sexton concludes that the operation suggested by Bonnet and others, establishing a permanent opening in the drum-head, is not likely to prove as frequently useful as was at first anticipated. He further believes that the Politzer air-bag is sometimes liable to do injury by unduly stretching the *membrana tympani* when atrophied by disease.—*N. Y. Med. Record*.

COMMENTARIES ON DISEASES OF THE KIDNEYS.*

PART II.

By ARTHUR WYNNE FOOT, M.D., Univ. Dubl.; Senior Physician, Meath Hospital and County of Dublin Infirmary; Fellow and Ex-censor, King and Queen's College of Physicians in Ireland; Diplomate in State Medicine, Trinity College, Dublin; Lecturer on Practice of Medicine in the Ledwich School of Medicine and Surgery; Fellow, Royal Geological Society of Ireland, &c.

[Continued from page 280, Vol. LXVII.]

PROPHYLAXIS OF RENAL CALCULI.—*Congestion of the kidneys, active and passive.* Toxic agents which provoke acute renal hyperæmia—characters of the urine in—Treatment of active renal hyperæmia. Passive or venous congestion—a condition secondary to other lesions. Symptoms of passive renal hyperæmia—characters of the urine. Treatment of passive congestion of the kidney. *Parenchymatous nephritis*—its synonymous terms—causes. Acute parenchymatous nephritis after scarlatina—different from the febrile albuminuria in the acme of the disease. Diphtheritic nephritis analogous to scarlatinal—supposed to be due to emboli of bacteria. Acute nephritis from “catching cold”—more severe and prolonged than from other causes. Symptoms of acute nephritis—characters of the urine in same.

WE have already alluded to the treatment of the symptoms arising from the passage of renal calculi. There is another object to be kept in view in the treatment of nephrolithiasis—namely, to prevent the formation of renal sand and gravel, and in this consists the prophylaxis of renal calculi. To carry out this indication, it is of the first importance to regulate the diet properly. The great prevalence of this disease among elderly persons in the higher ranks of life, who indulge in a quiet, sluggish method of living, subsisting chiefly on a meat diet, would go to prove that bodily inactivity, together with a rich nitrogenous diet, constitutes at least one factor in the etiology of urinary calculi. It seems to have been proved beyond possibility of doubt that there is an increased excretion of uric acid when the amount of animal food consumed is augmented, yet an abundant consumption of nitrogenised food often does no harm when there is a corresponding consumption of the material of the body, such as takes place when the mode of life is active, and especially when much bodily exercise in the open air is taken. It is sufficient, then, to prohibit too excessive a use of meat, to recommend the

* The text of the essays of Professor Carl Bartels, of Kiel, and Professor Wilhelm Ebstein, of Goettingen, in Volume XV. of the “Cyclopædia of the Practice of Medicine,” edited by Dr. H. Von Ziemssen, has been followed as closely as possible, with additional notes from all the best authorities on the subject.—A. W. Foot.

use of white meat, and to insist particularly on great moderation in eating, and on the use of the most digestible articles of food. An exclusively-milk and vegetable diet may be recommended for persons in whom, in consequence of luxurious living, the excess of uric acid is very considerable. Butter and sugar, and all fluids containing sugar, should be avoided. Pure spirit and water is the best form of stimulant; the weak spirit and sugar of half fermented beer and effervescing wine more easily undergoes acid fermentation than fully fermented wine and pure spirit. A simple therapeutic measure, but one of the greatest importance, is the abundant use of soft spring water night and morning; it is the best diluent of the urine, it diminishes the irritation of the kidneys and the urinary passages, delays the precipitation of the uric acid, retains the solid matters in general of the urine in solution, and enables the constantly forming precipitates to be more easily washed out of the kidneys and the renal pelves. Alkaline mineral water, such as Vichy or Seltzer water, may be substituted for spring water, and taken not only with the food, but two hours before each meal. Some consider the waters that contain Glauber's salts—in the first rank of which must be placed those of Carlsbad and Tarasp—possess a marked superiority over the pure soda waters, and act more certainly and reliably. Alkalies, however, must not be given in such large quantities as to give the urine an alkaline reaction. If this should occur, a precipitation of the earthy phosphates from the urine within the urinary passages will unavoidably take place. In this way phosphatic concretions are formed, or incrustations of phosphates are deposited upon other varieties of calculi. Warm baths are to be used as aids to other treatment. Keeping the bowels open was recommended by Sydenham, who took a cathartic (manna and lemon-juice) upon a fixed day in every week for several months in succession, and found it always give him relief. Among the special agents which are considered to possess salient powers over uric acid are lithia, which exceeds all other bases in its power of dissolving uric acid, and phosphate of soda, which can dissolve more uric acid than most other salts.

Congestion of the Kidneys.—Congestion of the kidneys is an increase in the amount of blood in the vessels which, if sufficiently intense, gives rise to the passage of albumen, blood, and even fibrinous casts of the uriniferous tubules into the urine. In the kidney, as in other organs, we must distinguish between *active* or *passive* congestion, regarding the former as the introductory stage of inflammation, the latter as the result of some mechanical hindrance to the escape of blood. Active congestion or acute hyperæmia of the kidneys is produced by undue determination of blood to these organs, and when it occurs apart from being the anatomical commencement of a parenchymatous inflammation, we find it is a condition which arises almost solely as the result of some toxic influence. Cantharides is about the best known of those poisonous substances which, after being absorbed into the system,

finally provoke hyperæmia of the kidneys, and whether it be administered internally, or applied externally in the form of a blister, or as an ointment to keep up a discharge, acute symptoms of renal hyperæmia have been observed to follow its employment. Very large mustard plasters, when these have been applied to an extensive surface of skin, are known to exert an action upon the kidneys altogether similar to that produced by cantharides. In such cases the effect is no doubt produced by the irritant principle, whether of Spanish fly or of ethereal oil of mustard seed, being actually carried to the kidneys in combination with the blood. Besides the oil of mustard, the oil of turpentine is the only other volatile oil which is of any practical importance in regard to the production of renal hyperæmia; this it produces both when absorbed through the digestive canal, and when inhaled in the form of vapour, if only enough of it gain access into the system. Cubebs and copaiba may be included among the irritants which can induce renal congestion of an acute kind. The anatomical changes produced in the kidneys by cantharides, oil of mustard, and oil of turpentine are but little known, since but few opportunities are afforded of studying them in the dead body. The only thing that may safely be taken for granted is that both kidneys will be equally affected. At the *post mortem* examination in cases of poisoning with cantharides, the most characteristic lesion found is the frequently excessive cystitis, a circumstance which has led many to believe that the kidneys themselves were not principally affected by this poison. After death by cantharides, the urinary organs are not the only ones which show signs of inflammation; there is always evidence of inflammatory action throughout the digestive apparatus.

Setting aside the action of renal irritants, active renal hyperæmia may arise from excessive filling of the vascular system, as after abundant drinks; from hypertrophy of the left ventricle; from the use of diuretics; from compression of the abdominal aorta below the renal arteries; also from obstruction to the circulation in the capillaries of the skin from exposure to cold. In all these cases the hyperæmia chiefly affects the arterial supply of the Malpighian bodies. The chief symptom is an increased secretion of urine; the urine is more dilute, paler, and of diminished specific gravity. If the blood pressure in the Malpighian bodies becomes still greater, the urine is albuminous or even bloody. The symptoms of renal hyperæmia provoked by the irritants before mentioned set in only after a certain period of time has elapsed, and consist at first in a constant and urgent desire to pass water, although no notable increase in the quantity passed may be observed. The urine excreted is in many instances found to be very bloody, and more or less albuminous; it generally has a few casts in it. After the use of cantharides it occasionally happens that the urine contains such an enormous quantity of fibrin that if it happen to coagulate within the bladder the resulting clot is large enough

to prevent micturition. The spontaneous excretion of fibrinous coagula is especially characteristic of the action of cantharides, and the formation of transparent jelly-like masses in the vessel containing the urine may lead to the discovery of the clandestine employment of cantharides. Dropsy is not a symptom proper to renal congestion; when present it depends on other causes, commonly on lung or heart disease. Mere hyperæmia of the kidneys does not produce a fatal issue, although its persistence may induce permanent alterations in the tissues of the organs, and thus lead to an ultimately fatal result. One of the most important consequences of long-continued renal hyperæmia appears to be an excessive production or overgrowth of the interstitial connective tissue, which eventually passes on to contraction and atrophy.

The treatment of simple hyperæmia of the kidney is, in the first instance, directed to the removal of or abstention from the noxious agent which excited it; and after this the most efficient means of combating active renal congestion are complete rest of the body in bed, cupping the loins, brisk purgatives, the warm bath, and other diaphoretics; mucilaginous drinks, opium, milk and water, and a mild vegetable diet. In congestive hyperæmia the result of the injudicious employment of cantharides, internally or externally, the administration of camphor in doses of from two to five grains every three or four hours, has been highly recommended.

The passive or venous congestion of the kidney is not a spontaneous independent malady, but always the result of some serious disturbance of the circulation. As conditions inductive of this congestion there is, in the first place, that general stasis of the venous circulation throughout the body which is produced by valvular lesions of the heart and by certain affections of the lungs; and in the second place, such obstructions as exercise compression upon the vena cava ascendens above the entrance of the renal veins, or even upon these veins themselves. Foremost in frequency, as illustrations of the former class of cases, are obstructive valvular lesions at the mitral orifice, and such lung diseases (as emphysema and certain forms of interstitial pneumonia) as lead to extensive obliteration of capillaries in the pulmonary parenchyma. Illustrative of the latter class are those cases in which a gravid uterus or other tumour presses upon the emulgent veins or the upper course of the inferior cava. Sometimes a cirrhotic liver compresses the latter vein as it lies in the hepatic fossa. It is only the higher grades of general venous stasis which exert any appreciable effect upon the kidneys and their functions, and these higher grades of general venous congestion are most commonly produced by heart diseases.

In describing the symptoms associated with congestive (venous) hyperæmia of the kidneys it is not possible to exclude some that are produced by the fundamental malady from those that pertain to the functional disorders of the kidneys. These symptoms consist, on the one hand, in

a general overfilling of the veins, advancing even to well-established cyanosis; and, on the other, in diminished fulness throughout the entire arterial system—an emptiness sufficient to render the radial pulse small and feeble, and, in extreme cases, even imperceptible. The symptoms, in short, are those of extreme elevation in the tension throughout the venous system, with a corresponding diminution of tension throughout the arterial system. Attention is usually not directed to the renal functions until the heart defect has existed for some time, until cyanosis of the face indicates accumulation of venous blood in the peripheral veins, until dyspnoea on exertion, and orthopnoea distress the affected individual, and then dropsy, beginning with anasarca of the lower extremities, leads to a urinary examination. In such a case one finds that the quantity of urinary water excreted daily falls far below the normal average. This scanty urine is usually of a dark, brownish-red colour, and, although clear when first passed, quickly becomes thick, through an abundant deposit of urates; its ordinary reaction is strongly acid, its specific gravity high, reaching to 1030 or 1035. The urine, as might be inferred from this high specific gravity, contains a large quantity of solid constituents, among which the urea occupies the first place. The urine in heart disease, too, as a rule, is much richer in uric acid than that furnished by healthy individuals under ordinary conditions, both in percentage and in absolute quantity. This depends not upon the condition of the kidney, but upon the influence which the disturbance of the respiratory functions in heart disease exerts upon the tissue-interchanges and the products issuing from these. As soon as dropsy has set in, the urine constantly becomes albuminous, but the quantity of albumen, as a rule, remains small, seldom reaching to two parts per thousand. Coincidentally with the appearance of albumen, the sediment presents small, pale, homogeneous casts, although never in large quantity. There is no pain or even any decided tenderness on pressure in the region of the kidneys in venous hyperæmia, and it is in accordance with general experience that uræmic symptoms are not associated with heart disease unless some complication coexist. These disturbances of the renal functions in persons suffering from heart disease, when once established, may persist without any remission up to the end of life, and death is then usually ushered in by continually advancing dropsy and increasing difficulty of breathing, provided no accidental complication cut the case short. In other examples of heart disease the functional disturbances of the kidneys subside again completely, and often quite suddenly, too, as soon as the patient is removed from unfavourable conditions and subjected to skilful and appropriate treatment.

The diminution in quantity of the urine in the venous congestion of the kidney, in heart disease, is simply attributable to the diminution of the arterial tension, for upon the restoration of the natural pressure

throughout the arterial system, there at once ensues an abundant secretion of urine, and the actual quantity of fluid drained off by the kidneys may be largely in excess of that which has been taken in drink or otherwise ingested with the food. The albuminuria of heart disease is regarded by Bartels as the consequence of the abnormal increase of blood pressure in the renal veins, and he assumes that the albumen does not pass through the capillaries of the Malpighian tufts, but is forced through the walls of the intertubular capillaries directly into the urine tubes. The albuminuria comes and goes in heart disease with the appearance and disappearances of the cyanosis, and simultaneously with the depression and elevation of the arterial tension. The non-occurrence of uræmic accidents in the functional renal disorders provoked by venous congestion is due in part to the circumstance that in the passive hyperæmia of the kidney the epithelial lining of the urine tubes remains entire and sound, preserves its normal properties, and continues to fulfil its function of separating the products of nitrogenous waste from the blood; and that although the quantity of water separated by the Malpighian tufts is reduced to a minimum, it still suffices to eliminate the excrementitious urinary constituents from the blood, and to prevent any accumulation of them either in this or in the tissues. This is verified by the clinical observation of patients suffering from heart disease, in whom often very large amounts of the specific urine constituents are found in small quantities of urine. Again, in the consideration of this matter—the non-occurrence of uræmic symptoms in cardiac congestion of the kidney—it is to be remembered that in this last stage of heart disease the tissue interchanges, for evident reasons, are seriously interfered with, and that consequently only insignificant quantities of excretory material are formed, of which quite a considerable quantity may be lodged in the dropsical effusions, and thus remain outside the blood and lymph vessels. The treatment of passive congestion of the kidney turns upon that of the original malady to which it is secondary. Since in most cases there exists an irreparable fundamental disease—for it is the essential renal complication of mitral valve disease—attention must before all things be directed to the action of the heart. The violence and rapidity of the heart beats, which so often interfere with the complete filling of the left ventricle in mitral deficiency, may be subdued with certainty by digitalis. In other cases the feebleness of the heart's action may call for the most powerful stimulants, for under these conditions stimulants become the best diuretics. In cases of chronic cardiac affection with secondary venous hyperæmia of the kidneys in which digitalis is indicated, the neutral potash salts in infusion of digitalis often have a beneficial action. With the view of removing water from the system by the promotion of perspiration, the hot air bath is safe and efficacious, but, if inapplicable, prolonged hot water baths may be used daily. When the diaphoretic method fails to restrain the dropsy,

and drastic purgatives are inadvisable or unsuccessful, there remain but mechanical measures for the evacuation of the dropsical effusions.

Parenchymatous nephritis.—A large and important class of renal affections are comprised under the head of “parenchymatous nephritis.” This term is practically synonymous, among several others, with acute Bright’s disease, inflammatory dropsy, or acute renal dropsy, scarlatinal kidney, acute desquamative nephritis (Johnson), acute tubal nephritis (Dickinson). It may be stated generally that this inflammatory affection arises from unnatural stimulation of the kidneys. As a consequence of over-work, or of work to which they are not adapted, they take on an abnormal activity; they become congested, the tubes get choked up with epithelial growth, and the disease is established. The causes, which are numerous, are thus classified by Dickinson :^a—

1. Circumstances which throw upon the kidney the work of other glands; cold to the body by checking perspiration; obstructions to the escape of bile; destruction of one kidney by throwing double work on the other (?).

2. Diseases which develop a material which acts as a renal irritant—scarlatina, diphtheria, measles, erysipelas, typhus, pneumonia, and acute rheumatism.

3. Matters taken from without which act as renal irritants—turpentine, alcohol, cantharides, arsenic, &c.

The second of the above categories embraces a number of causes in which certain *specific* noxious substances are carried by the blood current to the kidneys—substances which irritate these organs, and eventually cause them to become inflamed. Among all the *specific* causes of acute nephritis scarlatina is the one which is most common. In childhood it is by far the most frequent cause of the disorder. It was long before those who in earlier times observed the occurrence of nephritis in connexion with scarlatina became aware that the kidney complication was of specific origin, and even at the present day there are persons who hold the view that the renal inflammation that follows scarlatina is provoked by cold, to which the skin, rendered more sensitive by the disease, is exposed. It would appear that in the course of scarlatina, as well as in certain other febrile diseases, notably diphtheria, morbid products are left in the blood which the kidneys take a share in removing. The kidneys are irritated by the poison which selects them as a mode of exit. It is the experience of all who have had a large acquaintance with scarlatina that the most severe renal affections may occur in the course of convalescence from scarlatina in cases where the parents have never allowed their children to leave their beds, while neglected children, who could be said to have passed through their attack of the disease, absolutely in the streets, running about almost naked in snow and rain, remain

^a Diseases of the Kidneys. Part II., p. 270.

entirely free from such sequelæ. The view that the kidneys are irritated by a morbid product derives support from the common observation that where the throat suffers severely, and seems to bear the brunt of the *materies morbi*, the kidneys are mostly exempt; yet in individual cases it does not seem to be the intensity of the general or local symptoms of the scarlatinal process which determines the occurrence or the absence of the renal affection. This is often absent in cases of the greatest severity, while on the other hand it may lead to a fatal issue after a perfectly mild scarlatinal attack—so mild as not to be noticed as such at the time, or to be suspected prior to the appearance of dropsy. The frequency of nephritis after scarlatina varies remarkably in different epidemics, and seems to be entirely dependent upon the character of the epidemic. As a general statement nephritis is more common in those epidemics which have been called malignant by reason of the severity of the febrile symptoms and the dangerous complications of all kinds (especially those of a diphtheritic nature) that attend the individual cases.

Care must be taken not to mistake the effect of the febrile action in its production of albuminuria for the specific influence of the scarlatina process upon the kidneys. Febrile albuminuria occurs in the severe form of scarlatina, just as it does in every other form of grave febrile disturbance, but then it coincides with the height of the fever, and subsides with the fall of the temperature. The specific nephritis does not manifest itself in the course of scarlatina until after a period of almost complete remission from all febrile action of from one to two weeks' duration has elapsed and the eruption has quite disappeared. A febrile albuminuria appearing at the height of the exanthem, lasting for a few days, and disappearing again with the subsidence of the fever, does not by any means imply that a nephritis will follow. The albuminuria, which signifies the existence of a renal inflammation, appears at a much later date, and in the majority of cases first shows itself about the twentieth day from the first appearance of the rash. Bartels, however, has been able to determine the commencement of nephritis as early as upon the tenth day of the scarlatinal disease. Some writers have asserted that every case of scarlatina is attended by an affection of the kidneys, but careful and exact microscopical and chemical examinations of the urine in a really large number of cases of scarlatina show that such is not the case. In far the larger proportion of all the cases of scarlatina, albumen, according to Bartels, never occurs in the urine, and throughout the whole course of the complaint neither renal epithelium nor its fragmentary *débris* are discoverable in the urine. Up to the present all attempts have failed to discover any substance in the urine after scarlatina that in its transit through the kidney, like a poisonous irritant diuretic, could act as the provoker of the inflammation. It is quite unlikely that the pathological changes which take place in the skin in scarlet fever can of themselves

be the cause of the renal inflammation. Far graver alterations of the skin following a no less acute or febrile course occur in confluent small-pox without being usually followed by nephritis. When renal inflammation follows a skin affection, like an extensive burn, for instance, the nephritis is provoked at the height of the malady, not after the subsidence of the pathological process in the skin, as is the case after scarlet fever.

Diphtheria also, like scarlatina, entails acute inflammation of the kidneys, but also, like scarlatina, does not excite a renal affection in every instance. The severity also of the renal affection stands in no direct relation to the extent of mucous membrane involved in the diphtheritic process, or to the intensity of the general disease. Oertel has undertaken to show that the special cause of the diphtheritic nephritis is the penetration of micrococci into the kidneys, and their multiplication within these organs; yet other most competent observers have failed to confirm the views of Oertel as to the production of the diphtheritic nephritis by emboli composed of bacteria.

Among the first class of the causes of acute nephritis given above we find included the comprehensive group of causes which is comprised in the phrase "catching cold." Although catching cold must frequently be offered as an etiological scapegoat, its importance as a cause of inflammation of the kidneys cannot be doubted. The illustrations of its action offered by Bartels are typical and of frequent occurrence. One patient got drunk after spending a winter night in dancing, threw himself, half-undressed, on his bed by an open window, and went to sleep. When he woke up his limbs were stiff with cold. From that hour he felt sick—a few weeks later his whole body was dropsical, and his urine was strongly stained with blood. Another patient, while perspiring freely from severe work, left his smithy, and went into the open air, with no covering on his shoulders except a shirt, and, while cooling a piece of red-hot iron in water, allowed himself to be wet through with an icy rain. Two weeks later he was brought into hospital, suffering from dropsy and hæmaturia. A third, immediately before his illness set in, broke through the ice while skating, and only succeeded in extricating himself from his cold bath after prolonged exertions. Whatever views may be held as to the way in which inflammatory affections are caused by exposure to cold, there is one point in which they all agree, and that is, that the violent contraction of the cutaneous vessels produced by extreme cooling of the skin must drive the blood into the internal parts of the body, and consequently lead to elevation of the blood-pressure in the vessels of the internal organs. The inflammation of the kidneys and of other organs consecutive to extensive burns of the surface of the body, is produced in the same way as the inflammation which is due to catching cold. It is well known that the destruction of the skin produced in this way, when extensive, causes a general depression of the temperature of the body, in consequence of

the great loss of heat; it acts in the same way as a continued abstraction of heat by cooling of the uninjured skin.

From what has been stated as to its etiology it will be seen that acute nephritis may owe its origin to a great variety of causes; but, however originating, the anatomical and clinical manifestations of the diseased processes are strikingly similar, and no matter what the cause of individual cases may be, the anatomical changes in the kidneys, and the clinical symptoms, are as liable to be slight as severe. It is, however, well established that the duration of the acute nephritis which results from severe cold is more prolonged, and its detrimental influence on the general health is more intense than in the majority of the cases of scarlatinal nephritis.

Acute nephritis may be ushered in in one case with symptoms of high fever, attended with alarming retching and vomiting, while in another there may be no fever at all; even in these latter cases extreme anæmia and great loss of strength are noticeable in a very short time. The first effects of renal disturbance are exhibited in diminution or complete suppression of the urine, and in the appearance of albumen and usually also of blood in the fluid. Bartels has observed that with the exception of a few instances of choleraic nephritis, every case of acute parenchymatous nephritis in which complete suppression of the urinary secretion took place terminated fatally, even when small quantities of bloody urine were again passed after prolonged suppression. In very mild cases, all evidences of the disturbance of the renal functions may disappear in the course of a week or two. Such mild cases of nephritis are not uncommon after scarlatina, are the ordinary rule after diphtheria, and are also frequently met with in connexion with other exciting causes. In the more severe cases, when the inflammation has caused a more profound alteration of the condition of the renal vessels, recovery never sets in rapidly, and the affection very often terminates fatally. The nephritis which is due to catching cold, as a rule, presents this more violent type. In very acute cases death takes place after the patients have become more or less dropsical—usually during uræmic convulsions, or in the coma that succeeds them. When a case of acute nephritis follows a slow course, it leads, in the majority of cases to dropsy, which is sometimes slight and sometimes excessive, and which may last for months, subsiding and reappearing at intervals. The dropsy itself may be the immediate cause of death, either in consequence of excessive accumulation of fluid in the serous cavities of the body, or by an acute œdema of a large portion of the pulmonary tissue, or, in rare cases, by acute œdema of the submucous tissue of the larynx. However, the great majority of all the cases of acute parenchymatous nephritis terminate in recovery. With regard to the length of time that elapses before the disease terminates in recovery there are

variations in different cases which evidently depend on the nature of the exciting cause of the renal affection. While a scarlatinal nephritis that lasted two months would be considered exceptionally long, an acute nephritis, due to a severe cold, is apt to be much more protracted, and sometimes continues more than six months. The rarest termination of acute parenchymatous nephritis is its conversion into a chronic renal affection that continues for years. The cases of acute nephritis that originate from cold are the most liable of all to develop into a chronic affection.

When nephritis sets in in an acute form the patient may within a few hours become "ill all over," with shivering and headache. Soon his face becomes puffy, particularly about the eyes, and oedema spreads quickly over the body. He has a dull pain or feeling of weight in the loins, and the urine has nearly stopped. If the nephritis be very intense the urine may be reduced to 2 oz. or 3 oz. a day, and that deeply tinged with blood. The pulse is hard and full, the skin hot and dry, the tongue coated, the face flushed, total loss of appetite, great thirst, and perhaps vomiting. The suppression of urine cannot last long. If the secretion do not speedily increase, the patient will be poisoned by the retained urinary matters, become comatose, and die. It is exceptional for nephritis to occur in this rapid and active form. In the majority of cases the disease, though it begins with marked and definite symptoms, runs a slower and longer course. The head symptoms which occur in the more prolonged form of the complaint are usually of a convulsive kind, whereas in the more acute form coma is apt to set in without any such prelude. The epileptic seizures sometimes come on without any premonitory sign, or they may be preceded by pain in the head, drowsiness, or peculiarity of manner. The convulsive seizures may be repeated in quick succession, and then pass off without any further mischief, or they may give place to a condition of incomplete coma, which is apt to end fatally. Continued vomiting appears to act as a forerunner of the convulsive attacks. Much as children are liable to convulsive seizures during the course of other diseases, it appears that in acute nephritis they are less liable to be affected by head symptoms than grown people. Dickinson* remarks that it is worth observing, in connexion with the state of the brain in these attacks, that they almost always occur with dilated pupils, whereas during congestion of the brain the pupils are generally contracted. When the head symptoms take a severe form, the attacks occur in quick succession, sixteen or seventeen sometimes happening within as many hours. The pallor of the countenance will often serve as a ready means of distinguishing head symptoms from this cause. If the disorder take a favourable turn the urine will increase in quantity in some cases even to twice or thrice the natural amount. The

* *Diseases of the Kidney. Part II., p. 275.*

dropsy will at the same time subside, the albumen at length cease to be perceptible, and the patient will be convalescent, though often left in a very anæmic condition. Recovery is often heralded by a most surprising increase in the quantity of urine—as many as twelve imperial pints may be passed in the twenty-four hours, carrying away with it not only the dropsy, but a large excess of urea, the result of previous accumulation.

The changes which the urine undergoes in acute nephritis concern principally its quantity and its chemical composition. Its quantity suffers a notable diminution in the early and progressive stages, and sometimes a great increase during recovery. The more scanty the urine is, the more intense the disease, and there is no more promising sign in nephritis than a spontaneous increase in the amount of the urine. When the disease occurs in a severe form the urine may be reduced in quantity to between half an ounce and two ounces daily. This is the minimum, and only occurs in cases where the tubes are almost universally stopped up with epithelium or fibrin. The degree of diminution reached in such instances is fortunately rare, since it is a symptom of the worst omen.

The chemical composition of the urine in nephritis is affected by the addition of albumen, and sometimes of the crystalloid constituents of blood, and of blood in its entirety, and by the diminution of all the natural urinary components. Albumen is invariably present in abundance. The commencement of the disorder is generally marked by a decided coagulum of albumen in the urine—a fact not observed with other forms of renal disease. It affords by its quantity a generally correct measure of the severity of the attack. A diminution in the amount of albumen is as favourable a sign as an increase in the quantity of water. The quantity of albumen discharged may be stated to vary from a trace up to 35 grammes (more than an ounce) of dry albumen in the twenty-four hours.* The means of making an exact estimate of the daily loss of albumen are both troublesome and tedious. The increase or diminution from day to day may be sufficiently ascertained by the rough method of boiling, acidulating with nitric acid, and measuring with the eye the bulk of the coagulum. This varies from a mere cloudiness, such as is noticed during convalescence, to a bulky clot, which in some cases is large enough to convert the whole quantity of fluid operated upon to a gelatinous or solid mass.

Dickinson draws attention to the recognition by means of guaiacum of the crystalloids of blood as among the early signs of the disease—one which may, indeed, precede the appearance of albumen in the urine. A state of urine in which the crystalloids only of the blood are discoverable has been described as a pre-albuminuric stage of albuminuria; and it would seem that at least in some cases the sapphire blue imparted to the urine by its admixture in a test tube with tincture of guaiacum and

* Dickinson. *Op. cit.*, p. 288.

ozonic ether declares the presence of those elements of the blood before the less fluent albumen has been able to traverse the coats of the vessels. The following is a convenient way of employing this test:—A piece of white filtering paper is first wetted with a tincture of guaiacum; on to the wetted portion two or three drops of the suspected urine are then let fall; and, lastly, a little solution of peroxide of hydrogen, or of the so-called ozonic ether, dropped on to these. A faint or bright blue stain developed at the spot where the urine had fallen will soon indicate the presence of blood.* This reaction (which results from the oxidation, by the peroxide of hydrogen, of the resin deposited from the tincture in the presence of hæmoglobulin), according to Mr. Mahomed, accompanies a general increase of arterial tension, which precedes the local renal inflammation and constitutes a stage of the disease in which it is amenable to preventive, however obstinately at a later epoch it may resist curative, measures. The excretion of urea is diminished as long as the disease is stationary or on the increase. The amount of urea generally varies with the amount of water, and when the latter is very scanty the urea is proportionately reduced and may fall to a twentieth of the normal amount. This extreme diminution is a symptom of the worst import, and is usually followed by nervous disturbance—generally convulsions. When the urine is allowed to stand in a tall glass cylinder a copious sediment is thrown down, particularly when the complaint is of recent origin. In the absence of blood the sediment will consist of the contents of the tubes. There will be seen multitudes of cells of renal epithelium, which may be natural or fatty, and in advanced cases a few pus-corpuscles may be detected. Furthermore, there will be casts which may occur in such great abundance as to form by themselves quite a palpable sediment. There will be found, especially in the early periods of the disease, epithelial casts which consist of a cylinder of coagulable matter, studded over with epithelial cells, which adhere thereto and are partly embedded therein. Sometimes these casts seem to consist entirely of compacted epithelial cells, or of epithelial cells held together by fibrin, so small in amount as to be barely perceptible. The coagulable material which forms the basis or stroma of a cast is separated from the blood in the renal tubes which act as moulds to the nascent material and impress it with their size and shape. Whatever matters the tubes may contain will become embedded in the stroma as it coagulates. It is in the course of acute inflammation of the kidneys that the epithelial lining of the renal tubes may be shed or desquamated in their natural continuity, and appear in the urine as epithelial casts. If the epithelium passes off, not in entire cells but broken up, the casts are *granular* in character—often opaque and coarse. These are as truly epithelial in their structure as those where the cells are seen entire and distinct. The granular casts generally appear in the more advanced

* *Clinical Reports on Renal Diseases.* William Carter. 1878, p. 212.

stages of the disease, since it is necessary for their production that the epithelium remain in the tubes long enough to become disintegrated. There are also observed in the sediment simply fibrinous casts, small when from a tube duly lined with epithelium, large when from one which has lost its epithelial growth. During the presence of hæmaturia the casts may contain blood-corpuscles, or may have a brown colour from being tinted with hæmoglobulin.

[*To be continued.*]

REMEDIES FOR DILATING THE CERVIX DURING AND PREPARATORY TO LABOUR.

At a meeting of the Obstetric section of the New York Academy of Medicine, Dr. Sell related a case as an example of several, in which he used the concentrated tincture of caulophyllum, or squaw weed, with the happiest results, as a remedy to ward off tedious labour. The remedy was especially applicable in those cases in which the woman had habitually suffered severely during the first stage of labour. As a preparatory remedy in such cases it should be administered in twenty-drop doses three times a day, for three or four weeks previous to confinement. Dr. Merrill remarked that he had witnessed similar results from the use of castor oil during labour. He referred to cases in which he had found the os rigid, had ordered a dose of the oil, and, by the time the bowels were freely evacuated, the rigidity had disappeared, and speedy delivery was effected. He further remarked that he had used castor oil with good effect in cases in which the uterine contractions were weak, and the os was considerably dilated. Given in half-teaspoonful doses every ten or fifteen minutes, the oil had produced marked uterine contraction as rapidly as he had ever obtained by the use of ergot. Dr. Sell regarded the oil as a dangerous medicine to be given during the latter months of pregnancy, for if it had the power to excite uterine contraction, it might produce premature delivery. He also referred to gelsemium as a valuable remedy in cases of rigid os during labour. Dr. Merrill remarked that he never ordered castor oil, except when he was convinced that it was time for labour, or the process had already commenced. He always charged his patients not to take castor oil during the last months of pregnancy, if remedies were needed to keep the bowels open, because of the liability to excite uterine contraction. Dr. F. V. White remarked that when he was an intern in Bellevue Hospital, it was customary to administer castor oil to the lying-in women on Sundays, and there were marked results following its administration. He also asked Dr. Sell if he had not obtained as satisfactory results from the use of chloroform in cases of rigid os during labour as from the use of gelsemium? Dr. Sell replied that he preferred gelsemium to chloroform.—*N. Y. Med. Record*, Jan. 25, 1879.

THE DOCTOR SOCIETY FOR MEDICAL OBSERVATION

SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P.

VITAL STATISTICS

*Of Eight Large Towns in Ireland, for Four Weeks ending Saturday,
February 22, 1879.*

Towns	Population in 1871	Births Registered	Deaths Registered	DEATHS FROM ZYMOTIC DISEASES							Annual Rate of Mortality per 1,000 Inhabitants
				Small-pox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhoea	
Dublin, -	314,666	791	1162	86	29	27	9	8	46	11	48·0
Belfast, -	182,082	502	567	—	1	18	2	24	28	20	40·5
Cork, -	91,965	203	205	—	—	—	—	1	5	2	28·8
Limerick, -	44,209	111	193	—	—	—	—	18	1	7	56·5
Derry, -	30,884	70	69	—	1	—	1	—	3	—	29·3
Waterford, -	30,626	63	44	—	—	—	1	—	—	—	18·8
Galway, -	19,692	51	44	2	—	—	—	—	1	—	29·3
Sligo, -	17,285	23	39	—	—	—	—	3	1	1	29·3

Remarks.

An excessive mortality prevailed in Limerick and Dublin. In Belfast also the death-rate was very high. In Derry, Galway, Sligo, and Cork it was high; but in Waterford it was low. The rate of mortality per 1,000 of the population annually was 25·1 in London, 24·3 in Edinburgh, 26·7 in Glasgow, and 50·0 within the municipal boundary of Dublin. Omitting the deaths of persons admitted into public institutions from localities outside the district, the death-rate of the Dublin registration district was 45·9 per 1,000 annually. Of the total deaths registered in Dublin (1,162) 243, or 20·9 per cent., were ascribed to zymotic diseases, which had caused 137·4 deaths on the average in the corresponding period of the previous ten years. Small-pox and whooping-cough were as fatal as in the first four weeks of 1879, while measles, scarlatina, diphtheria, and fever all show an increased fatality. A serious epidemic of whooping-cough is prevalent in Belfast, Limerick, and Sligo. In Belfast scarlatina, fever, and diarrhoea are destructive to life. Of the 46 deaths attributed to fever in Dublin, 9 were due to typhus, no fewer

than 32 to typhoid, and 5 to continued fever of undetermined type. An outbreak of "milk typhoid" in the S.E. part of Dublin and the Pembroke township has been clearly traced to a dairy, where a typhoid fever patient lay ill for some weeks during December and the commencement of January. At the close of the four weeks 177 small-pox patients were under treatment in the Dublin Hospitals, compared with 188, 122, 86, and 47, at the close of the four preceding periods respectively. The excessive mortality from diseases of the respiratory organs continues in Dublin. The deaths in the four weeks were 391, or 33·6 per cent. of all the deaths. They included 312 from bronchitis and 42 from pneumonia. The ten years' averages for the corresponding period were—respiratory affections generally, 191·8; bronchitis, 155·4; and pneumonia, 18·5. It will thus be seen that the mortality from these diseases was more than double the average. During the first eight weeks of 1879 the registered deaths in Dublin were 746 in excess of the registered births.

METEOROLOGY.

Abstract of Observations made at Dublin, Lat. 53° 20' N., Long. 6° 15' W., for the Month of February, 1879.

Mean Height of Barometer,	-	-	-	29·500 inches.
Maximal Height of Barometer (on 26th at 9 a.m.),	-	-	-	30·313 „
Minimal Height of Barometer (on 10th at 3 p.m.),	-	-	-	28·820 „
Mean Dry-bulb Temperature,	-	-	-	39·7°
Mean Wet-bulb Temperature,	-	-	-	38·3°
Mean Dew-point Temperature,	-	-	-	36·4°
Mean Elastic Force (Tension) of Aqueous Vapour,	-	-	-	·216 inch.
Mean Humidity,	-	-	-	88·5 per cent.
Highest Temperature in Shade (on 6th),	-	-	-	51·3°
Lowest Temperature in Shade (on 26th),	-	-	-	27·9°
Lowest Temperature on Grass (Radiation) (on 26th),	-	-	-	24·6°
Mean Amount of Cloud,	-	-	-	77·5 per cent.
Rainfall (on 23 days),	-	-	-	3·706 inches.
General Direction of Wind,	-	-	-	E.S.E. and W. by N.

Remarks.

A very wet, dull, and rather cold month, the mean temperature being some 3° below the average of February in the preceding thirteen years (42·6°). Atmospheric pressure also was considerably below the average. On the 5th a complete change of weather occurred. The polar air-current disappeared temporarily from Western Europe, and a series of atmospheric depressions began to travel north-eastwards along the W. coasts of Ireland and Scotland, causing generally fresh S.W. winds, a copious rainfall, and considerably warmer and softer weather. On the 8th nearly an inch of rain (·940 inch) fell in Dublin. During the week

ending Saturday, the 15th, also, very broken, dull, and rainy weather was experienced in the United Kingdom. This was caused by the passage eastwards across Western Europe of several depressions, while the barometer was comparatively high, and the isobars were anticyclonic in the North of Europe, where intense cold prevailed. Up to the 15th rain had fallen on every day in Dublin, to the total amount of 3·357 inches. The weather subsequently became colder and somewhat drier, the third week being characterised by a deficiency of atmospherical pressure, variable winds, a low temperature, and frequent falls of snow and sleet. The distribution of pressure over Europe during this period was very remarkable. Throughout the week the barometer remained relatively high over Scandinavia and the Mediterranean, while a trough of low pressure extended from N.W. to S.E. across the British Isles and Central Europe. Along this line of barometrical depression a series of bourrasques travelled south-eastwards, bringing with them changeable weather and much cold rain or snow. Two snow-storms occurred in Dublin on the 19th, and the weather remained cold until the 26th, when a depression appeared in the N.W., causing a S.W. wind and rapid rise of temperature. In Dublin the thermometer stood 17° higher at 9 a.m. of the 27th than it had done twenty-four hours previously. On the 28th, however, the wind veered to N.W., and temperature again fell fast, the month closing with a clear, frosty, moonlight night. A solar halo appeared at 3 p.m. of the 8th, a lunar halo on the evening of the 4th, and a lunar corona at 11 p.m. of the 3rd. The weather was foggy on the 2nd, 8th, 9th, 10th, 11th, and 21st. Snow or sleet fell on the 1st, 18th, 19th, and 24th; hail on the 1st, 21st, 22nd, 23rd, 24th, and 25th. There was a little soft hail or "Graupel" at 3 50 p.m. of the 23rd.

TREATMENT OF BALDNESS.

THE following is highly commended by Dr. George H. Rowe in the *Atlantic Medical and Surgical Journal*, for seborrhœa and consequent alopecia. It is the plan of Professor Kaposi: R. Saponis viridis (German), alcohol, āā. f. ʒ ij.; solve, filtra, et adde ol. lavandulæ, gtt. xx.—xxx. Pour one or two tablespoonfuls upon the scalp, then pour on a little water, rub smartly with the fingers, thus producing a copious lather. After four or five minutes' shampooing this way, rinse the head thoroughly with pure water, dry thoroughly with a towel, then apply a little cosmoline. This process causes the hair to fall out in greater abundance at first, but a new and fine growth of hair soon follows.—*Lond. Medical Record*, March 15, 1879.

THE BOSTON
SOCIETY FOR
MEDICAL
OBSERVATION
PERISCOPE.

Edited by G. F. DUFFEY, M.D., F.K.Q.C.P.

OVARIOTOMY.

AN instructive case in which the wall of a misplaced bladder was involved in the incision made in this operation is reported by Dr. Leroy M'Lean. The patient was a young lady twenty-four years of age. Marked anteflexion of the uterus could be detected by vaginal examination. Before proceeding to the operation a large aspirating-needle was introduced to the left of the median line, and a quantity of fluid, having the consistency and colour of molasses, withdrawn. The bladder was then evacuated. The incision was commenced half an inch below the umbilicus, and extended down two and one-half inches. At the lower angle of the incision, at a depth of three-quarters of an inch from the surface, Dr. M'Lean cut into what appeared to be a cyst in the abdominal walls, which contained about two drachms of pale fluid. The edges of the incision did not retract as they usually do when the abdomen is tense, and not liking the indications presented, and not being thoroughly satisfied as to what he had to deal with, the incision was carried upwards to a point on a line and to the right of the umbilicus, when the unmistakable ovarian sac was reached. Using the finger as a director, the opening was completed below, and the tumour removed. Then was discovered that the bladder had been cut through on its anterior and posterior surfaces, down to the lower end of the incision. The anterior surface was strongly adherent to the abdominal wall. It was not adherent to the tumour. In completing the incision from above downwards the finger used as a director had passed behind the bladder (its walls being then in *close contact* from pressure of tumour behind), and the injury done. The bladder was repaired with interrupted silk sutures. The adhesions of its anterior surface to the abdominal walls were not disturbed. Abdominal incision closed with silver wire. A soft rubber self-retaining catheter was introduced and left *in situ*. Although an hour elapsed after the operation before this instrument could be procured, two ounces of urine were withdrawn on introducing it. She died thirty hours after the operation. The secretion of urine was normal. The report of the *post mortem* is as follows:—Primary union of abdominal incision; no evidence of peritoneal inflammation; the bladder intact, good primary union, and no escape of urine into the cavity of the abdomen. There was no evidence of sloughing of the pedicle. The adhesions of bladder to abdominal wall so firm that they cannot be separated without tearing that viscus. The uterus was ante-

flexed and bound down by strong adhesions. The only theory Dr. M'Lean offers in explanation of the malposition of the bladder is, that the uterus having been anteflexed to the degree it was, the bladder under distension would naturally enlarge in the direction where the least resistance was offered. It is well known that women will sometimes go for many hours without micturating. It may have been so with this patient, and as a result the distended bladder crowded itself upwards between the tumour and the abdominal walls, and was held there mechanically by pressure of the tumour, after it had been emptied. It may be that the condition was a congenital one. Had the tumour being adherent to the bladder there might be no doubt as to the cause of the anomaly. It might be suggested, as a wise procedure hereafter, to pass a sound into the bladder and the exact condition of that organ ascertained before operating.—*N. Y. Med. Record*, Feb. 8.

TROUSSEAU'S CATAPLASM.

DR. DIEULAFOY (*Lyon Méd.*, January 26, 1879) who has frequently applied this cataplasm with much success, gives the following directions for its preparation:—Take, according to the size of the affected articulation, 3 or 4 lbs. of bread—4 lbs. are sufficient for the knee-joint, 2 lbs. for the wrist. Cut it into pieces, removing carefully the hard portions of the crust, and soak the bread for about a quarter of an hour in water. It is then taken out, tied into a cloth, and squeezed to express a part of the water absorbed, so that the bread remains moist, but not too wet. It is then put into a steam-bath, and allowed to remain there for three hours, when it becomes like dry paste, which is softened by the addition of camphorated alcohol. This dough is then kneaded for about five minutes, till it is of the consistence of plum-pudding. This is the most delicate point in the making of the cataplasm, because if it is too soft it will give way, and spread out under the pressure of the dressing, and if it is too hard it is apt to crumble and break into small pieces, which might injure the skin. The degree of consistency of the cataplasm must, therefore, be very carefully supervised, because unless one is in the habit of making it, there is always a tendency to make it too soft, either because the bread has not been squeezed sufficiently before having been put into the steam bath, or because too large a quantity of camphorated alcohol has been poured upon it. The dough, having thus been prepared, is spread on a linen bandage in the shape of a rectangle, large enough to cover the whole of the joint. The poultice must be at least $\frac{1}{3}$ of an inch thick at the edges, in order to prevent the thinner portions from drying too quickly. The surface of the cataplasm is then painted with the following mixture: camphor, 7 grammes; extr. opii, 5 grammes; extr. bellad., 5 grammes; alcohol, q. s. This being done, it is applied by being put over the affected joint, and covered by non-evaporant covering.

The whole is then firmly fixed by means of a long flannel bandage, over which is placed a linen one of the same length. These bandages vary in length, according to the size of the joint, and, consequently, to the size of the poultice. The joint having been thus bandaged, it must remain perfectly immovable; the compression, although firm, must not cause the underlying parts to become œdematous; this may be prevented, however, by bandaging them also. In order to prevent the layers of the bandages from slipping, they must be sewn to each other. The cataplasm then remains in the same position for eight or ten days, after which time it is removed, and found to be as fresh and moist as if it had been just applied; it still smells of camphor, and does not present the least trace of mould. The skin which has long remained in contact with it is perfectly healthy, unless the cataplasm should have been too thin at the edges, thereby either drying too soon, or giving way under pressure of the bandage, and causing the skin to excoriate. This is Trousseau's cataplasm. At first sight it may appear too expensive for poorer patients, because the cost of the material amounts to from two-and-sixpence to five shillings if the appliance is made in a hospital. If, however, we consider that, the expense having been once incurred, the cataplasm remains in its place for at least eight days, during which time no other medicine is given, we are soon convinced that it is even cheaper than most other appliances. The indications for the use of this cataplasm are so obvious that they need not be repeated here. In every kind of chronic or sub-acute inflammations of the joints, when other means, such as blisters and cauterisation, have proved unsuccessful, and even in the first instance, Trousseau's cataplasm will be found most useful and advantageous.—*Lond. Med. Record*, March 15, 1879.

THE PONTRESINA BOY.

DR. R. TUTHILL MASSY communicates to *The Scientific Review* (February, 1879), the following account of the "Pontresina Boy":—"During my recent wanderings on the Continent, and while taking a morning walk from the Hotel Saratz, where I resided at Pontresina, to the 'Five-cornered Tower,' my attention was attracted to a strange-faced boy of the Mongolian type, with great width across the eyes, to the prominent temples and zygomatic arch on each side, which gave the features a vacant, meaningless expression. The remarkable flatness above the nose gave to this central organ a Wellingtonian outline. I beckoned him towards me, and by a little bribery got off his soft hat, and then saw and felt his extraordinary head, which reminded me forcibly of the Doge of Venice's crown reversed. Next day I had a chat with Dr. Ludwig, the resident physician, and with his tact succeeded in getting a photograph. Both parents objected to have any exhibition made of this boy, but, as they had another younger—considered perfection—they finally consented

to have the two grouped together, but not separate, and for this sitting they were to be presented with six copies. After the first photograph was taken I managed to get the elder boy's side face before the camera, and thus got this excellent profile (see Fig. 1). The mother assisted us here in pressing down his dark hair with her saliva and making the deformity more visible in the cranium. On my return to Brighton I called on Mr. Edmund Wheeler, the photographic portraitist, to separate and enlarge the full face (see Fig. 2) to correspond with the side view,

Fig. 1.

Fig. 2.

and thus we are indebted to this art for the exact likenesses now before us. The boy is said to have a mathematical mind, at school keeps his place among the classes, and has a good character from the master. I have noticed this lad when playing on the road with bigger boys to be their leader. His age is thirteen years. The circumference of his head across the forehead and temples measures twenty and a-half inches. The greatest height from the ear-openings over the elevation is fifteen and a-half inches. From the same points behind, the elevation, over the lowest part, measures twelve inches. This description will, I hope, give our phrenologists a distinct idea of this unique head, and the physiognomists an equally distinct idea of a Chinese face. As far as we could ascertain there was no skull-shaping in infancy. The mother thinks the deformity is much less, and the outline has greatly improved, the last few years. Neither the father, Hartm Lorenz, nor the son, Luzin, have the physiognomy of the Saracenic type, which is oblong. The mother has a

cast of that kind in the outline and expression of her features, which perhaps she has inherited from the Saracens, who built the tower about the tenth century to watch the traffic over the Bernina Pass. The entrance door is high up, like those in the Round Towers of Ireland, and would require a ladder to see the inside. The birds have taken up seeds of the Cembran pine, for now this tree can be seen flourishing over the battlements. Its medicinal properties are especially antiscorbutic."

A NEW METHOD OF REMOVING SUBMUCOUS AND INTERSTITIAL FIBROIDS OF THE UTERUS.

At a late meeting of the New York Academy of Medicine, Dr. T. Gaillard Thomas read an important paper upon this subject. The method consisted in seizing the most dependent and accessible part of the tumour with a strong vulsellum forceps, passing along its sides a *serrated scoop, or spoon-saw*, and by a gentle pendulum motion from side to side, sawing through the attachments of the tumour and forcing it entirely from its connexion with the uterus. The advantages claimed for the instrument were the following:—1. The attachments were separated by a saw which greatly limited the hæmorrhage. 2. The shape of the spoon, convex without and concave within, caused it to follow of itself the contour of the tumour, and at the same time protect the uterine tissue. 3. The highest attachment could be as readily reached as the lowest. 4. The saw action secured separation with rapidity and with certainty. 5. The spoon-saw secured separation of the growth at its highest point of attachment, and left no peduncle to decompose. To illustrate the advantages which the new method possessed over the old methods, a number of cases were first reported, and then followed by the report of a number of similar cases operated upon by the method just described. A description was also given of a new instrument, and a new method of determining the extent and situation of the attachment which the tumour had to the uterine wall. After trying various methods, he had fixed upon the use of a *flat whalebone sound*. In order to ascertain the outline of the tumour and the extent of its attachment, the index finger of the left hand was placed against its most accessible part, then the sound was passed up along the side of the tumour until it became arrested. The sound being then withdrawn and the finger kept upon it, it was laid upon a sheet of paper, and, being curved, a line was drawn from its tip to the indicating finger. The same was done upon the opposite side of the tumour, and in that way an approximate and wonderfully exact idea could be obtained with reference to the situation and extent of the attachment.—*N. Y. Med. Record*, Feb. 8.

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THE DUBLIN JOURNAL

OF

MEDICAL SCIENCE.

MAY 1, 1879.

PART I.

ORIGINAL COMMUNICATIONS.

ART. XV.—*American Experiences of Animal Vaccination.* By
CHARLES CAMERON, M.D. Dubl.; M.P. for Glasgow.

THE last published volume of the Transactions of the American Medical Association (Vol. XXVIII.) contains many interesting papers, but none, perhaps, of so much interest and importance to the profession in this country as that of Dr. Martin, of Boston, on "Animal Vaccination." Dr. Martin has for many years made a specialty of vaccination, and is an acknowledged authority on the subject. In 1870 he procured from Professor Depaul, of Paris, a supply of animal lymph of the Beaugency stock, and proceeded to propagate it on a herd of calves with which he had provided himself for the purpose. The experience, of which he gives the result in the paper just referred to, extends over a period of six years and nine months. During that time he had vaccinated and superintended the vaccination of over 600 heifers. From these animals 800,000 charged points, besides a large supply of fluid lymph, were procured, and distributed to thousands of physicians over the length and breadth of the States. Cities and towns threatened with epidemics had been supplied by him wholesale—occasionally with as many as 15,000 points; and, on one occasion, an Icelandic colony in British America being threatened with destruction from a smallpox epidemic, he furnished it with 3,000 points. Besides

a mass of correspondence which, under the circumstances, Dr. Martin must naturally have had, and which must have given him an unrivalled insight into the general experience of his brother-practitioners regarding animal vaccination, he himself performed nearly 12,000 vaccinations and revaccinations with animal lymph. The results, therefore, of the experience of so practised and observant a vaccinator should carry with them very great weight, and well deserve the consideration of the medical profession in Great Britain.

Sooth to say, our English physicians have paid very little attention to the subject. In 1870, Dr. Seaton, one of the medical officers then attached to the Privy Council Medical Department, and now on the staff of the Local Government Board, published his well-known report on the subject, in which he pooh-pooed the whole thing, and the mass of the profession in Great Britain have been content to let the matter rest where he left it. But the literature and philosophy of the whole subject have made very considerable strides since that time. Whereas one of the main objections urged by Dr. Seaton against the practice was that it was impossible to rely on a continuous propagation of calf lymph, Dr. Warlomont in Brussels, and Dr. Martin and others in the United States, have, since the date of his report, without any difficulty, kept the supply going for seven years. Again, the serious objection was urged that the animal vaccine was uncertain in its operation—more difficult of “taking” than humanised lymph. Experience has demonstrated the fallacy of this assertion. It requires a little knowledge and a little care, certainly, to ensure success, but with proper precautions experience has proved that animal lymph will, in cases of primary vaccination, show quite as good results as humanised lymph (Dr. Martin alleges even better), while in revaccination, according to Dr. Martin’s experience, there is no comparison between the two. And Dr. Martin ought to speak with authority on the point, for during the American war, in 1860, years before the system of animal vaccination was thought of in America, he revaccinated over 5,000 recruits for the army. The results were carefully noted, and the number of cases in which the operation was successful amounted to about 33 per cent. Of course, in these cases, the lymph used was the ordinary Jennerian article. But with animal vaccine he claims that the successes on a single trial have amounted to 75 per cent.; and, if those cases in which a second revaccination

succeeded after the first had failed are included, that the proportion of successful cases amounted to a shade over 80 per cent.

Before going any further it may be as well to explain exactly what constitutes what is termed animal vaccination. Spontaneous cow-pock is a rare disease, and to vaccinate from it would be obviously impracticable. Every medical man knows the importance of selecting lymph for the purpose of vaccination at the proper stage of development of the vaccine vesicle; but when cases of cow-pock are discovered, it is impossible to name the precise date of the eruption. Moreover, the cow is liable to a variety of eruptive diseases which closely simulate genuine vaccinia in outward appearance, but have really no connexion with it. But having secured a single case of undoubted cow-pock, nothing is easier than to propagate the stock by inoculation on young and healthy animals; and experience has shown that so far from deteriorating in the process, the lymph, after one or two transmissions, acquires a more benign and uniform action, while it retains its full vitality and specific characteristics for an indefinite series of years.

Those instances of vaccination direct from the spontaneous disease of the cow, observed by Jenner and the older writers on vaccination, are described as having been accompanied by symptoms so severe and inflammatory that the practice was proscribed. This is, however, nothing more than one would have expected from the analogous case of variolous inoculation. Every one who has dipped into the literature of that subject must be aware of the precautions which inoculators took in the selection of their virus, and the serious results which occasionally occurred from their neglect. No inoculator, for example, would have dreamed of having recourse for his lymph to a patient suffering from a malignant form of small-pox; but if one has to trust to the cow for cow-pock, one has no choice. Happily by artificial means, by propagating the disease in young and healthy animals, there is no difficulty in securing it in its most benignant form. The result has been, according to Dr. Martin's experience--and in this it has but corroborated that of Dr. Warlomont--that there is no more danger of undue inflammatory action being set up by animal vaccine than by the operation of the ordinary humanised lymph. On the contrary, Dr. Martin maintains that the adoption of animal vaccine affords an absolute safeguard against the supervention of erysipelas, which, he maintains, is as purely human a graft upon the vaccine virus as syphilis

itself. The obvious advantages of animal vaccination, then, are that, wherever it is wanted, an unlimited supply of pure lymph can be obtained, and that by means of calf lymph the vaccinator can give those interested an absolute guarantee against the invaccination of any of those human affections which are occasionally communicated in operating with the ordinary vaccine. To the physician who knows the extreme rarity of any complication of this sort, the latter consideration may have little weight; but the bare possibility of the invaccination of syphilis, which is too well substantiated for denial, and which is sedulously magnified by anti-vaccinators into a matter of every-day occurrence, constitutes, in the minds of a large proportion of those of the laity who give the subject a thought, a consideration which causes many of them to suspect vaccination altogether, and which prevents thousands from availing themselves of the practically absolute protection against smallpox which revaccination affords. The importance of the immunity in this respect which is offered by animal vaccination may be gauged by the fact noted by Dr. Warlomont, that in Belgium, on the smallest alarm of smallpox, the numbers who present themselves for revaccination have greatly increased since the introduction of the system of animal vaccination. Now, for the stamping out of smallpox, revaccination is quite as important as primary vaccination, and any means of encouraging the one is of as vital moment to the community as the enforcement of the other. But there is another advantage presented by animal vaccination on which Dr. Martin lays the greatest stress. In the early days of vaccination a single submission to the operation was held to confer as absolute freedom from smallpox as a previous attack of that disease itself. This is now admitted not to be the case. Was Jenner wrong, or has the vaccine lymph, in the course of repeated propagations through the human system, lost something of its vigour and prophylactic effect? The highest medical authorities in this country deny that it has; but, on the other hand, some of the very highest authorities in some of the foremost of Continental countries—the Academies of Medicine in France and Belgium, for example—have endorsed the contrary opinion. And experiment appears conclusively to have proved that the cow-pock virus, while capable of being propagated in several species of animals, does undergo modifications, more rapid in some, less so in others. Thus the elaborate experiments of M. Chauveau and his colleagues showed that cow-pock inoculated upon the horse reproduced itself with less and

less vigour, until after half a dozen removes it ceased to reproduce itself at all. More than this—they showed that while one could, with absolute certainty, reckon on the success of a vaccination in a horse with lymph direct from the cow, vaccination with humanised lymph invariably failed to produce any effect upon that animal. In man, therefore, as well as the horse, cow-pock, according to the Chauveau commission, appears to go through a process of attenuation and degeneration. Man, however, seems to furnish a soil, so to speak, on which the vaccine virus can be propagated for a long series of years. That now in use in this country has been in constant circulation through the human system for eighty years. Dr. Martin admits that, owing to the care which has been exercised in the choice of vaccinifers, it is the best to be found in the world. But has it changed? He maintains that it has, most materially, and he quotes the early vaccinators and Bosquet in support of his assertion. The course of the vaccine vesicles resulting from the long humanised virus and from animal vaccine, or vaccine only a few removes from the cow, is, up to a certain point, identical, so that on the seventh or eighth day it would be impossible to distinguish one from the other; but, after that point, the characteristics of the two are perfectly distinct. In the case of the old humanised lymph the aureola fades and the vesicle dries up, and in the course of another week the scale is ready to drop off. When animal vaccine, or vaccine only a few removes from its original source, is employed, on the other hand, the aureola goes on increasing in extent and deepening in colour for some days after the first week, the vesicle continues developing for several days longer, and the scab does not separate till the twenty-first day, or sometimes for a considerably longer period. Again, in the ordinary vaccine vesicle familiar in this country, only the most superficial tissues of the skin are engaged. In the case of animal vaccine the vesicle burrows deep into the skin, and the result is a much firmer vesicle of a pearly lustre, justifying literally Jenner's poetical description of it as "a pearl resting in a rose-leaf." Dr. Martin mentions a stock of vaccine which he used for some years, which regularly ran its course in eleven days. The Jennerian stock, which he obtained from the Cow-pock Institute in London, took fourteen days to run its course, while the animal vaccine required for its maturation twenty-one to twenty-eight days. Again and again, he tells us, he introduced these various stocks upon the same patient, and in every case the vesicles remained true to the type of the lymph from which

they had respectively been derived. From this Dr. Martin argues that by a long course of transmission through the human organism the vaccine virus as unquestionably degenerates, as it does so much more rapidly in the horse. Hence, he alleges, the discrepancy between the prophylactic effect of vaccination as observed in Jenner's day and at the present time; hence the fact that re-vaccination, with strong, undegenerated animal lymph, will prove successful in 40 per cent. of cases where the operation might be practised in vain with the weakened Jennerian lymph—at present practically the only stock available in this country; and hence the fact for which Dr. Warlomont vouches—that during the various epidemics of smallpox which have occurred in Belgium during the eleven years in which he has practised animal vaccination, not a single case of that disease has been known to occur in any person who had been vaccinated with animal lymph.

ART. XVI.—*On Thoracic Aneurism Simulating Mediastinal Cancer.*^a By THOMAS WRIGLEY GRIMSHAW, M.A., M.D., Univ. Dubl.; F.K.Q.C.P.; Physician to Steevens' Hospital, &c.

THE difficulties which surround the diagnosis of intra-thoracic tumours are so great that any case which may tend to throw light upon the question must be considered of value.

In *The Dublin Medical Journal* for May, 1870, I published a paper, read before the Medical Society, "On a Case of Malignant Disease of the Thorax and Stomach." In that paper I described a case where a diagnosis of thoracic aneurism was made by an eminent physician, but the disease proved to be mediastinal cancer. In a case recently brought before the Medical Society by Dr. Finny,^b a case of malignant disease within the thorax also simulated aneurism. The case which I am now about to bring under the notice of the Society was diagnosed by me, in the first instance, as a mediastinal tumour either of a strumous or malignant character. As the case proceeded I arrived at the conclusion that it was one of mediastinal cancer, involving the upper part of the right lung, pressing upon the laryngeal nerves, and finally infecting the glands of the right side of the neck, and probably producing extensive secondary deposits in the thoracic viscera. The case was as follows:

^a Read before the Medical Society of the King and Queen's College of Physicians in Ireland, on Wednesday, April 2, 1879.

^b See *Dublin Journal of Medical Science*, Vol. LXIII., page 328. April, 1877.

Mary H., aged sixty, occupation a vegetable dealer (she carried a basket of vegetables on her head when pursuing her occupation), admitted to Steevens' Hospital on October 14, 1878. She had, a year before, been under my care for a short time, when she suffered from an attack of acute bronchitis, with an unusual amount of laryngeal irritation. On that occasion she left the hospital before she was completely well, and I expressed an opinion that she had something more than bronchitis—probably enlarged bronchial glands. On admission the second time she complained of a choking sensation, with violent fits of paroxysmal cough and profuse expectoration. Her face presented a slightly puffed appearance; her paroxysms of coughing were generally brought on by swallowing; she said her food tickled and choked her, but did not hurt her when she swallowed. During each coughing fit her face became congested, and the superficial veins on the thorax became distended. She stated she had suffered from the cough for six weeks before admission, and had come to the hospital because "she had been cured the last time." The patient was much emaciated, although she stated she had a fair appetite, and had taken plenty of food. A physical examination of the chest discovered a loud laryngeal stridor; over the rest of the chest sibilant and sonorous râles were to be found in various places; respiration was generally diminished. On the right side of the sternum a rough systolic murmur was audible over the second intercostal space. This murmur was limited to a small area. On percussion a certain amount of dulness was detected extending from the left margin of the sternum to the middle of the right clavicle in front and downwards for between $2\frac{1}{2}$ to 3 inches. The inter-scapular space behind was duller than normal, but there was no discernible difference in the percussion note on the two sides. No difference was observable in the size of the pupils, and no enlarged glands could be detected in the neck. The sputum was extremely viscid, and frequently streaked with blood. Dr. Bookey, on several occasions, minutely examined the sputum microscopically, but nothing specific could be discovered.

On October 15th two leeches were ordered to be applied over the top of the sternum, and 20 grains of bromide of potassium were administered every third hour with a view of relieving the spasm. This treatment appeared to produce some amelioration of the spasmodic cough. The patient remained in much the same state for some time; the puffiness of the face seemed slightly to increase.

On November 3rd she became sleepless at night, and chloral was ordered at night in combination with bromide of potassium.

On November 8th there was observed for the first time a swelling about an inch and a half wide above the middle of the clavicle on the right side of the neck; it was soft and elastic, and gave the impression to the finger of a cyst containing air. On pressing deeply around thi-

tumour, some hard and tender masses—apparently indurated glands—were to be felt.

On November 9th the swelling appeared more diffused, but well marked.

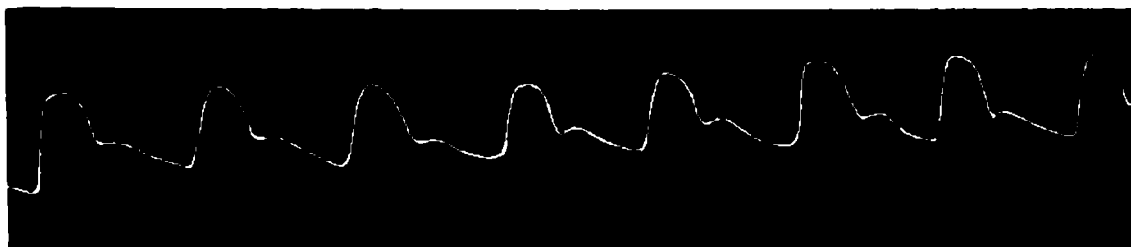
On November 13th the tumour had become hard, and extended for about two inches along the margin of the clavicle, and upwards for about an inch and three quarters; it was distinctly nodulated on the outer aspect. The tumour had a pulsating movement; this movement was manifestly communicated by the neighbouring vessels, and was not in the tumour nor distensile. There was no murmur audible over the tumour, and pressure upon it caused considerable and sharp pain. The patient gradually became weaker—she refused food, became comatose, and died on November 20th.

A *post mortem* examination was made twenty hours after death, when the following conditions were found:—Curvature of diaphragm normal; anterior mediastinum normal; both lungs slightly adherent at apices, and congested, especially the right; heart fatty; valves competent; cervical glands slightly enlarged. The internal jugular vein of the right side was enormously distended. The arch of the aorta was found to be dilated through its entire length, and contained atheromatous and calcareous deposits. The transverse portion of the arch and arteria innominata were involved in an aneurismal tumour. This tumour pressed upon the commencement of the superior vena cava and both the innominate veins at their junction with the cava. The junction of the subclavian and jugular veins of the right side was occupied by a solid coagulum extending upwards through the whole length of the internal jugular vein. The right pneumogastric nerve and its recurrent branch were both pressed upon by the tumour. The left pneumogastric and its recurrent branch were also pressed upon. The sac was occupied by a laminated clot, with a secondary clot extending to the aortic valves.

It will be observed from the foregoing account of the case that the only positive signs of aneurism present were dulness on percussion, a systolic blowing murmur, and signs of pressure within the chest—namely, laryngeal spasm and puffiness of the face; the latter, however, seldom appearing except after a severe fit of coughing. The dulness in the interscapular space—the wasting, bronchitic râles, and ropy expectoration streaked with blood—all appeared to me to justify the conclusion that the case was one of malignant disease of the mediastinum. The occurrence of the nodular swellings in the neck during the last fortnight of life confirmed this view. Aortic murmurs, in conjunction with intra-thoracic tumours other than aneurisms, are so common that the presence of the

murmur over the aorta rather tended to confirm than contradict my diagnosis. The pulse tracings (see Figs. 1 and 2), no doubt,

Fig. 1.



Tracing from Right Radial Artery, October 10, 1878.

Fig. 2.



Tracing from Left Radial Artery, October 10, 1878.

pointed to diseased arteries, but only of such a character as are frequently met with in persons of the age of the subject of these remarks. Indeed the condition of aorta met with on *post mortem* examination would fully account for the character of the sphygmograms even if a marked aneurism had not been present. For my own part I cannot at present see how I should make a more correct diagnosis if a case with identical symptoms were to come under my notice.

ART. XVII.—*Abdominal Cancer Simulating Aneurism.** By
ROBERT SAMUELS ARCHER, A.B., M.B., Dubl.

THE case about to be related presented so many of the phenomena usually found in connexion with aneurism of the abdominal aorta, that it occurred to me a short record of it might possibly prove not altogether uninteresting.

Abdominal aneurism is an affection which not infrequently presents many problems of diagnosis difficult of solution to the clinical observer. More especially perhaps is this the case in the earlier stages, before the sac has attained a size of sufficient dimensions to become manifest externally by a pulsating tumour, accompanied by a murmur, &c. However, even in these cases a more or less constant boring pain in the back, with periodical exacerbations, and a localised bruit occasionally heard (after careful examination) along the course of the spine, may indicate the true nature of the lesion, even at a very early stage of its

* Read before the Liverpool Medical Institution, April 3, 1879.

development, to say nothing of the detection of a dilated aorta by rectal examination.

An eccentrically pulsating tumour, in the immediate neighbourhood of which a murmur is heard, is apt too often to be regarded as aneurismal, without due importance being attached to the other circumstances and phases of the case. Identical symptoms do exist in some cases of tumour non-aneurismal, especially in those intimately connected with the aorta, and which are of a semi-fluid, or at least not of a very solid, nature. Such an one is the case, a short account of which I am about to give.

Not only may malignant tumours be occasionally mistaken in their earlier stages for aneurism, but also, *vice versâ*, may an aneurism present such characters as to simulate a circinomatous growth. This latter is a rare occurrence, but such an error in diagnosis may be possible if the aneurism be nodular, quiescent, and if the emaciation and cachexia be extreme. The late Dr. Stokes has placed on record a case of this latter description, in which the diagnosis was a matter of much uncertainty even to that distinguished and accurate observer of diseased conditions.

CASE.—Allen M'Cormack, a sailor, aged fifty years, was admitted to Mill-road Hospital, Liverpool, on June 5th, 1877. Had been intemperate, but never had syphilis. About two years and a half before he came under observation he had been crushed about the legs and the lower part of the trunk amongst the cargo in a ship's hold. Ever since then, according to his own account, he had felt himself "getting bad." Pulse was 66, regular, and equable. He complained of pain in the epigastrium, which was aggravated by taking food. He occasionally vomited matter, which at times, he said, "was like barm," but never brought up anything like blood. Tongue was rather coated. A distinct, diffuse, distensile, and heaving impulse could be felt in the epigastrium. This extended in every direction for about four inches, and a corresponding area of dulness could be indistinctly (owing to the intervention of coils of intestine) mapped out by percussion. A blowing murmur was heard over the seat of impulse. *Impulse persisted on placing the patient on his hands and knees, but was neither increased nor lessened.* Heart's sounds and action were in every respect normal. He was emaciated to a certain degree, and anæmic-looking. He was ordered bismuth and hydrocyanic acid to check the sickness.

June 7th.—He vomited some frothy, brownish fluid, with some flakes in it. Complained of pain over the seat of impulse. He was now given large doses of iodide of potassium, which seemed to relieve the pain.

22nd.—He again complained of much pain across the seat of impulse,

especially towards the right side. Some solution of morphia was added to each dose of the iodide.

July 2nd.—Pain under floating ribs on left side.

30th.—Suffered great pain across the abdomen; passed a fair night, but was occasionally awakened by “sudden jerks of pain.”

August 10th.—For some time he had been suffering intense pain at the area of pulsation and round the umbilicus, and also under the false ribs on both sides. There was now pain in the back, *but not of a boring or severe character.*

Sept. 1st.—Pulse over 100; suffers intense pain in the old positions, and has again been troubled latterly with the vomiting. Has become very much emaciated. Pulsation in epigastrium has grown much stronger and more distensile, and the tumour more prominent. No nodulation could be detected.

3rd.—There was an abatement in the force of pulsation.

13th.—He had become very much easier in every respect, the pains being much lessened. He found much relief by lying diagonally on his face and right side, the thighs and legs being flexed. A distinct bruit still persisted. He has emaciated even to a greater extent, is very anæmic, and his face has assumed a dirty, dingy-looking hue.

Oct. 24th.—During my absence on “fortnight’s leave” he had become weaker and weaker each day, and sank exhausted on this date—no new symptoms, as far as I can learn, having supervened.

25th.—*Post mortem* examination. Dead about twenty-five hours. Emaciation extreme; no subcutaneous fat whatever; rigor mortis absent.

Chest.—The lungs appeared to be in every respect healthy. Both pleural cavities contained a small amount of fluid. The heart was small and pale-looking; its valves and orifices were perfectly normal.

Abdomen.—The liver was considerably enlarged, its weight being $5\frac{1}{2}$ lbs. It was studded here and there over its surface with stellate, yellowish, somewhat puckered markings, scarcely raised above its general surface, and presenting on section a cheesy, friable-looking appearance. The entire organ was crowded with these nodular bodies, varying in size from a small hazel-nut to a small apple. Its upper and posterior aspect adhered to the diaphragm. Adhesions also existed between it and the small intestines. The duodenum was almost entirely ensheathed in a soft mass of encephaloid. Running along the pancreas to the hilus of the spleen was a chain of enlarged cancerous glands. The greater mesentery also was studded with these bodies. In front of the spine, lying on the aorta and almost encircling it, was seen a large, somewhat nodular mass. This extended from the coeliac axis, whose branches were seen piercing it, almost to the bifurcation of the aorta.

Remarks.—Let us now briefly review the principal symptoms

and physical signs in this case, which bore a strong resemblance to—in fact were identical with—those usually found in the majority of cases of aneurism.

(1.) In the first place, the history was very similar to what we are accustomed to hear in these cases—viz., a crush about the legs and lower part of the trunk, accompanied, no doubt, by violent struggles on the part of the patient to extricate himself from his uncomfortable position. Such like accidents, the pursuit of various trades which require the constant pressure to be exerted on the epigastrium, excessive muscular exercise or blows on the abdomen, are the exciting causes we most frequently read of in the history of abdominal aneurism.

(2.) The patient in the case we are considering dated his illness from the time of this accident, and the development of his symptoms seemed to have been gradual and progressive from that time.

(3.) Then we had an epigastric pulsating tumour, having a distinct, forcible throb, expansile in all directions, not merely the forward upheaval that one might expect to find in a case where a solid tumour was lifted, so to speak, from its bed by the pulsation of the great vessel lying behind it. This pulsation also was *persistent and equable in all positions*, which is characteristic of aneurism, or of a tumour most intimately connected with the aorta. The tumour was of a kind which would be likely to be affected by eccentric pulsation communicated from the aorta. It was one of those semi-fluid encephaloid growths which (as the late Dr. Stokes, in his work on “Diseases of the Heart and Aorta,” points out) are very apt to simulate aneurism—at least before nodulation can be detected, and the so-called cancerous cachexia has become developed.

(4.) A bruit, well developed, and heard whenever it was sought in all positions in which it was possible to examine the patient, both in front and posteriorly, was a physical sign highly presumptive of aneurism, occurring, I believe, in about 25 per cent. of the cases, but, I need hardly add, by no means peculiar to this affection. The remarks made above, concerning pulsation with reference to persistence and equability, are also applicable to this sign, and need not be repeated. Perhaps we may add here that the bruit accompanying an aneurism in many cases disappears on the patient assuming the sitting posture.

(5.) The pain was not quite of the kind usually met with in aneurism; it seemed to be situated more towards the anterior

aspect of the tumour, and, although it radiated round the lower part of the chest, it never was of the *boring character* in the back which is so frequently complained of. I thought this might be owing to absence of erosion of the bodies of the vertebræ. Nor did it shoot down one or both lower extremities. However, it was paroxysmal, and *appeared to be relieved* for a time by the use of iodide of potassium—a drug which is now much recommended in the treatment of aneurism, and whose beneficial action is supposed to be due to lessening of the blood-pressure in the sac.

(6.) Vomiting (which was also present in this case) is a symptom found in a small proportion of abdominal aneurism cases. I suppose it is produced either by reflex action through implication of the great ganglia of the sympathetic, or possibly by direct irritation of the stomach, in cases where this viscus is pressed upon by a tumour. This symptom may be present in any case of abdominal tumour—it was a most distressing and uncontrollable feature in a case of aneurism of the celiac axis which I published some years ago.*

(7.) The decubitus was somewhat characteristic of aneurism—viz., crouched up diagonally on one side, the legs and thighs semi-flexed, so as to relax the abdominal muscles. I have seen this position assumed in at least one case of abdominal aneurism. Dr. Stokes mentions this form of decubitus.

These seven features—viz., (1) the history of an injury, (2) the mode of onset of the disease, (3) the pulsation, (4) the bruit, (5) the pain, (6) vomiting, and (7) the decubitus—which I have thus endeavoured separately to analyse, were all highly presumptive of aneurism when taken in their entirety; and I think any physician might fairly be justified in coming to a *probable* diagnosis of this lesion from such data. Such diagnosis would be found to be correct in the majority of cases. However, experience would teach us to be cautious, and to refrain from giving a positive opinion.

In looking back at the case, the progressive and rapid emaciation, and the peculiar dingy appearance of the patient, ought possibly to have induced one to modify his opinion, but up to a fortnight before the patient's death, I do not think these were much more distinctive than what is commonly seen in aneurism. Certainly, in the absence of marked nodulation, they were quite insignificant as compared with the other phenomena. The patient died on the day I resumed duty at the hospital, after an absence of

* Irish Hospital Gazette. January 1st, 1875.

a fortnight, and my friend, Dr. Pollard, hinted at the possibility of finding malignant disease. The result of the *post mortem* examination proved this opinion to be correct.

Quite recently Dr. Scheele, of Dantzig, has drawn attention to the increase of local pain caused by compression of the femorals as a valuable diagnostic point in abdominal aneurism. Now, I think it quite possible, had it occurred to us to employ this test in the present case, an increase of local pain might also have been found to occur, and thus still further evidence in favour of a faulty diagnosis would have been forthcoming. This method of examination, however, cannot be generally recommended, as several instances of rupture of the sac have occurred, apparently produced by the increased blood-pressure.

THE PATHOLOGY OF ADDISON'S DISEASE.

IN the *Archives de Physiologie Normal et Pathologique*, 1878, Nos. 5 and 6, M. Jacquet arrives at the following conclusions :—1. In Addison's disease, the bronzed skin one finds only as a lesion of the sympathetic system, pigmentation without atrophy of the nervous cells of the ganglia which are in the neighbourhood of the diseased suprarenal glands. 2. The degeneration of a part of the nervous fibres attaching the same lunar ganglia to the nervous centres ought to be regarded as secondary and consecutive to the process of sclerosis which accompanies the tuberculisation of the capsules. 3. That lesion is insufficient to serve as the basis of a pathogenic theory of Addison's disease. 4. Hyperpigmentation of the nervous cells of the great sympathetic and of the cerebro-spinal system is a fact of the same order as the hyperpigmentation of the epidermic cells of the Malpighian plexus. 5. This hyperpigmentation renders probable the existence of an alteration of the blood by the substances which a suprarenal gland would, in the normal state, be employed in utilising by transforming them. 6. The alteration of the blood by functional or organic insufficiency of the suprarenal glands is a pathological phenomenon analagous to that which exists in chronic uræmia. 7. Alongside of the melanoderma, by alteration of the suprarenal tissue, there seem to exist cases in which the melanoderma is due to the lesion of other blood-making organs. 8. Clinical researches in Addison's disease ought especially to be directed to the chemical analysis of the blood and the urine.—*Lond. Med. Record*, April 15, 1879.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

La Chirurgie Journalière: Leçons de Clinique Chirurgicale, Professées à l'Hôpital Cochin. Par ARMAUD DESPRÈS, Professeur Agrégé de la Faculté de Médecine. Paris: J. B. Baillière et Fils. Pp. 689.

THE author of this book has adopted, with some modification, an old idea first carried out by Callison, in his *Systema Chirurgiæ Hodiernæ*, and has even put the title to the result of his labour. He addresses himself to the consideration of the every-day surgery of ordinary practice, discussing the forms and treatment of common diseases, believing that when one knows these he is very near a knowledge of exceptional surgery, which, after all, is not so exceptional as some believe.

In his first lecture he defines clinical study to be the application of surgical pathology, and he wisely observes that diagnosis will not be sound, prognosis will be uncertain, and the value of treatment be deceptive, if the surgeon does not know the natural course of disease. The *debut du mal*, as Nélaton called it—its progress up to the day on which the surgeon is called, constitute this natural course, and he then proceeds to illustrate this by several examples.

In his observations upon dressings, M. Desprès begins that criticism which is, throughout, a strong characteristic of his book. He warns his pupils against the enthusiasm of the authors of special forms of dressings, who would wish to raise their discoveries to the height of that of the circulation of the blood. The practice of drainage, as introduced by Chassaignac, and adopted by Lister, he regards with some doubt. He has seen patients treated with it who recovered neither better nor more quickly than if any other dressing had been used. He does not believe in the utility of the drainage tube in amputations, and thinks it should be confined to resections and disarticulation of the foot. Lister's method of dressing is very briefly alluded to; it is so minute, and requires so many special materials, that it is not to be recommended. The care

which a surgeon bestows upon the dressing of a wound is a guarantee of its success, but it is not necessary to push the manœuvres to puerility. Exorcisms, indeed, are all that are wanting in Lister's method. After all, it is only alcoholised dressing, with the method of Chassaignac.

A criticism of this sort will coincide with the views of many in this country, but not with those of the majority of surgeons. The first operators in Germany and America have tested the plan practically, and there can be no doubt that it has opened up vast possibilities in operations. It is right to say that the methods often practised, and called by Lister's name, are not his. The general neglect of small details is responsible for much of the non-success which follows the use of antiseptics. The very nature of the plan demands great accuracy and care, and if these cannot be bestowed by the surgeon he need not expect brilliant results. In France Listerism is not properly known or practised, and we are inclined to believe that M. Desprès would soon learn to become an admirer of the principles advocated by the great English surgeon if he would only carry out closely the directions given.

There are some interesting remarks upon what is called chronic contusion of the heel, occurring in persons who are required to keep the upright position for a considerable time. Policemen are subject to it, and we have ourselves seen two cases in members of that force. The examination of the heel reveals nothing particular—there is no swelling or redness; pressure is not painful; but there is sharp pain if we press with the tip of the finger upon the inferior face of the posterior tuberosity of the os calcis, being the precise place at which that osseous point bears upon the sole during the upright position. The patient describes his sensation as one of heat—even of burning.

Those with arched or small feet and delicate skin, and who sweat copiously, are most exposed to this affection. Another predisposing cause is the wearing of shoes of thin leather, and prolonged walks upon bad pavements. Fabre attributes the pain to an inflammation of the normal bursa situated under the calcaneum. But in the case in which this observation was made there were redness and swelling. Desprès adheres to his belief that it is chronic contusion of the parts, resulting from intermittent pressure.

The treatment consists of rest and the application of a metallic

sole moulded to the foot. This is placed in the shoe before the foot is introduced, and a very acceptable support is thus afforded.

In his lectures upon urethral stricture the author condemns the method of intermittent catheterism practised by Civiale, who, he says, treated all the diseases of the urinary passages "without having had the necessary preliminary medical instruction." He advocates, on the contrary, the *bougie à demeure*, which acts, not by ulceration or by dilatation of the fibrous tissues, but something in the same way that compression acts upon the granulations of an ordinary ulcer. This plan he places before every other. The numerous modifications of treatment he regards with great scorn. Thus, while some used the *porte-caustique* of Lallemand, others cauterised the stricture from before backwards, "while Civiale, in order to differ from his competitors, cauterised from behind forwards. Here again we have minutiae disputed with bitterness by specialists who thirst to appear as inventors of some plan their rivals do not possess. It is in inventions of this kind that the commercial side of specialists in medicine is best revealed." The method of gradual dilatation will do all that is necessary without risk. When we hear of strictures cured by intermittent catheterism, they are of the inflammatory kind. They may be treated in various ways; and it is in them that internal urethrotomy may be done without accident because there is nothing to cut.

So through all the book we find M. Desprès expressing himself with much vigour and freedom in criticising the teaching of others. But he is rather dogmatic, and is, perhaps, in another form, open to the charge which he brings against Civiale—that he is anxious to write something different from his contemporaries. The book, however, is one of much ability. It is fresh and crisp in its style, and very practical. Its incisive comments show how much there is to be said upon subjects that are by some thought to be past discussion.

The Cell Doctrine: its History and Present State. By JAMES TYSON, M.D. Second Edition. Philadelphia: Lindsay and Blakiston. 1878. 8vo, pp. 199.

• IN this work the author gives a short but fairly comprehensive history of the views which have been held as to the elementary parts of which the bodies of plants and animals are formed from the time of the *partes similes* and *dissimiles* of Aristotle up to

the present. Such a work is manifestly unsuited for analysis, and we would only notice that a very pronounced prominence is given to the theories of Dr. Beale, with whom the author agrees in most points, although he does not adduce any stronger proof than Dr. Beale has done of the formation of the cell body from the nucleus, and of this from the nucleolus. There is one point which has struck us in reading the work before us, and that is, that while the author acknowledges and takes account of nearly all the most recent advances in histology, he completely ignores the modern ideas as to the structure of the cells of connective tissue. Thus, at p. 100, he gives Robin's account of the formation of "the fusiform bodies which are the *connective tissue corpuscle*," and at p. 103 he states that a perusal of Robin's views "cannot fail to convince the reader of the accuracy of description of the fully formed elements described by Robin." Again, in giving his own views, and describing the various shapes of cells met with, Dr. Tyson says (p. 146)—"In the so-called *connective tissue* again we have a variety in the shape of the cells. Thus, in areolar tissue or connective tissue proper, we have young round cells composed almost wholly of germinal matter, exhibiting amœboid movements and all the characters of the leucocyte or colourless corpuscle, as well as the elongated spindle-shaped nucleated cell, so characteristic as to have long ago received the name *connective tissue corpuscle*. These latter cells also possess prolongations which unite with those extending from adjacent cells, and being hollow thus form a canalicular system, of exceeding fineness, which is believed to be capable of conveying nutrient juices in the absence of blood-vessels of sufficient size to conduct the corpuscles of the blood." At p. 143 also we find the following curious passage:—"In young connective tissue cells undoubtedly exist, with at least their more important parts as nuclei and protoplasm, but in fully formed fibrous tissue the only portion of the cell which I have been able to satisfy myself, in an almost daily study of healthy and diseased structures, is constantly present, is the nucleus." In the description of bone, p. 146, there seems to be a confusion between the lacunæ and the cells which lie in them, for it is stated that the cells have processes which "unite with those of neighbouring bone cells, and with the Haversian canals."

Cohnheim's observations are dismissed very briefly, and we think the present state of opinion is not fairly expressed by the following paragraph (p. 117):—"These observations of Stricker and Norris

are generally acknowledged as having settled the question in favour of the view that there are two elements of organisation—the colourless corpuscle and the connective tissue corpuscle, either of which may also become the starting-point of pathological new formations.”

We cannot but think that this utter want of appreciation of the change which the connective tissue question has undergone since the publication, in 1869, of Ranvier's classical paper on the Structure of Tendon is a serious blot on a book professing to represent the present state of the cell doctrine.

Forty-four pages of bibliography concludes the work. This, although making no pretension to completeness, will be found valuable. There are two plates, one copied from Dr. Beale, the other a reproduction of the figures accompanying Dr. Klein's recent paper on the Structure of Cells and Nuclei (*Mic. Jour.*, 1878).

1 *Manual for the Practice of Surgery.* By THOMAS BRYANT, F.R.C.S.; Surgeon to, and Lecturer on Surgery at, Guy's Hospital. With 672 Illustrations. Third Edition, revised and enlarged. London: J. & A. Churchill. 1879. 2 Vols., 8vo, pp. 728 and pp. 637.

WE have already (*Dublin Journal of Medical Science*, Vol. LXII., page 129) expressed a favourable opinion of the second edition of Mr. Bryant's "Manual of the Practice of Surgery;" and it gives us pleasure to state that the present edition fully sustains the reputation of the work. A careful revision of the whole manual has been made, and important additions are found in nearly every chapter. The illustrations, which are so attractive and—when well executed—so useful to the student, now number six hundred and seventy-two. Of them eighty-eight are new, and many are either tinted or coloured. We cannot imagine a more instructive series of diagrams than those illustrating the different operations for aneurism (Vol. I., p. 452), the various forms of arterio-venous aneurism (do., p. 459), the different forms of hernia (do., p. 642), and the varieties of clubfoot (Vol. II., pp. 314, 316).

The opening sentences of the "Introductory Chapter" in Volume I. exemplify the enlightened and liberal spirit in which Mr. Bryant has approached his task, and the intelligent view he takes of the science and art of surgery. He says:—

"Surgery is of a twofold nature. It is a science and also an art—a department that requires to be known and another to be practised. The

science embraces a knowledge of the character, the causes, and the effects of disease and injury, and also of the processes by which they are best repaired; the art the treatment of diseases or injuries as they present themselves. The latter, however, in order to be successful, must be based upon science, and science must be supported by observation at the bedside, where the phenomena of disease must be studied, and the symptoms recognised as well as duly weighed; and also in the *post mortem* room, where the effects are to be traced. The great object of the student, therefore, should be clinical and pathological investigation, the study of the living and the dead. It is by this alone he can hope to acquire a solid basis on which to ground his practice.

“The external features of a local disease may appear the same to the student as to the most accomplished practitioner; whereas the phenomena of disease are often perceptible to the surgeon alone—the acquired art of observation and the unconscious influence of experience giving to the mind of the one a power of recognition and interpretation which is denied to others.

“It is to the acquisition of this power, therefore, that the student should devote his energies, and to this end cultivate the art of observation, for by it he will not only gain the power of seeing correctly, but also of interpreting the meaning of what he sees, and will thereby acquire a diagnostic acumen which cannot otherwise be obtained” (p. 1).

It is true that now and then—as in the foregoing quotations—a certain uncouthness of expression and too frequent grammatical slips are calculated to irritate the critical or too sensitive reader of Mr. Bryant's manual; but no such shortcomings can destroy its popularity, and the author has evidently done his best to remove all blemishes of style. A more important consideration for the surgical reviewer is to ascertain whether the work really deserves the confidence of the profession and of students. We believe it does, and we heartily recommend it as a handy, reliable, and instructive epitome of modern surgery.

Drei Fälle von Gesichtsneuralgie durch Nerven-resection geheilt oder gebessert. Mitgetheilt von DR. C. FIEBER. Berlin. 1878.

THE three cases of facial neuralgia reported by Dr. Fieber (*Sep. Abdr. aus der Berl. klin. Wochensch.*, 1878, No. 19), in which improvement or cure followed resection of the nerve, are of considerable interest, especially as the author has had each of them under observation for several years after the operation. In the first case—one of extreme severity—neurectomy effected a perfect

cure, which has proved a lasting one since the performance of the operation seventeen years previously. In the second case, likewise, a complete cure was obtained; and, in the third case, a cure for two years was effected. The neuralgia then returned, and was benefited, but not perfectly relieved, by a second operation. These cases strongly support the views of those who advocate the utility of resection of nerves as an important therapeutic resource against the frightful agonies of neuralgia.

MR. SQUIRE'S *Scraping Spoon*.

MESSRS. WEISS & SON forward illustrations of the scraping spoons of Volkmann, and of Mr. Balmanno Squire's modification of them. The instruments employed by Mr. Squire are smaller, deeper, and narrower than the German spoons, and are well calculated for neat and delicate work. This mode of treatment—i.e., by erosion, is especially applicable to the various forms of lupus, rodent ulcer, scrofulous ulcers, and some other circumscribed chronic lesions of the skin.

An Inquiry into the General Pathology of Scurvy. By Dr. C. H. RALFE. Reprinted from *The Lancet*. 1877.

As the result of some inquiries and experiments carried out at the Seamen's Hospital (late Dreadnought), Dr. Ralfe thinks that he is justified in concluding that the fundamental fault in scurvy consists in a diminution of the alkalinity of the blood, produced in a two-fold way—viz., by an increase of acid urates in the blood, and by the withdrawal of salts having an alkaline reaction, chiefly alkaline carbonates.

De la Valeur Thérapeutique des Courants Continus. Par le DR. TEISSIER. Paris: Baillière et Fils. Pp. 176. 1878.

WE were much pleased with the perusal of this excellent compendium, which does not claim too much for electricity, and gives, in a readable and attractive form, a fair *résumé* of the existing state of knowledge. Our surgical readers will consult with profit the section on electropuncture in the treatment of aortic aneurism. It is carefully written, and the author contributes some original experiments upon the coagulating action of the current. At p. 142

is inserted a useful table of cases of aortic aneurism treated by this method. The author is cautious not to dogmatise upon a difficult problem, and he does not go too far in saying that in a disease so grave as aneurism, and one whose issue is inevitably fatal, the results obtained hitherto from electropuncture may be considered as relatively encouraging. We can recommend Dr. Teissier's essay as a candid and temperate account of the subject, so far as it goes.

Du Froid en Thérapeutique. Par le DR. F. LABADIE-LAGRAVE.
Paris: Baillière et Fils. Pp. 284. 1879.

THE aim of the author of this volume appears to be to place the therapeutics of cold applications upon a sound basis, and in Chapter I. he gives a fair and tolerably full account of the physiological effects of cold. The work is mainly a compilation, and contains nothing of importance that is not easily accessible to English readers in the writings of their own physicians, and in some of the volumes of the translation of Ziemssen's Cyclopædia.

GELSEMINUM FOR HECTIC.

PRACTICAL experience with gelseminum in small doses has long shown its influence upon the circulation and its sedative effect in certain neuralgias. It has also been shown to have a sedative effect upon the respiratory centres. From these facts it appeared to Dr. Edgar Holden that it should act favourably in the treatment of a respiratory affection characterised by irritation, as Dr. Holden believes the hectic of phthisis is, and having its seat and origin in the pulmonary tissues. In a very large number of cases it has not failed, and Dr. Holden has found that, even after the failure of favourite and well-known remedies, doses of two drops of fluid extract, or 10 to 12 of the tincture every two hours, will, in most instances, within forty-eight hours, arrest the chill, moderate the cough, and allay the fever. The period of administration, however, is not always so short. It may be used continuously if necessary to maintain sedation, and without interference with other medicines or effect upon digestion or the excretions. It should be added that exceptions are likely to occur in cases with mesenteric complication and colliquative diarrhoea, and while not contra-indicated, it may sometimes disappoint expectations.—*N. Y. Med. Record*, March 1.

PART III.

HALF-YEARLY REPORTS.

REPORT ON NERVOUS AND MENTAL DISEASE.*

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I. ANATOMY AND PHYSIOLOGY OF THE NERVOUS SYSTEM.

Observations on the Structure of the Brain of the White Whale (Beluga).—Dr. Herbert Major contributes to the *Journal of Anatomy and Physiology* a valuable record of his observations on the Histology of the Brain from a specimen of this animal which died some little time since in the Westminster Aquarium. The whole brain weighed 1,746 grammes (62 oz.) taken approximately, the cerebellum 226 grammes, the pons Varolii 37 grammes, and the medulla oblongata 6 grammes. The convolutions were traced out by Professor Turner, who states that “a well-marked Sylvian fissure was present on the outer surface of the hemisphere.” The convolutions were arranged around this fissure in four successive tiers, separated from each other by three well-defined fissures, which extended generally in the antero-posterior direction. On the inner surface of the hemisphere the convolutions presented considerable complexity, but they were obviously arranged in relation to the direction of the corpus callosum, and extended in the antero-posterior direction from the frontal end of the cerebrum backwards and downwards. There was evidence of a division of the convoluted mass into three successive tiers by intermediate furrows extending antero-posteriorly above the corpus callosum, and the convolutions of the middle tier were in greater mass than those of either the upper or the lower tier. The convolutions of the two lower tiers reached the temporo-sphenoidal

* The author of this Report, desirous that no contribution to the subject of Nervous and Mental Disease should remain unnoticed, will be glad to receive any publications which treat of it. If sent to the correspondents of the Journal, they will be forwarded.

part of the hemisphere, while those of the upper tier did not extend so far down, but stopped at the occipital end of the cerebrum. Regarding the relation of the minute structure to that of the human brain, Dr. Major found that the gray cortex of the cetacean is of less depth than that of the human cerebrum; the structural appearances presented by the cortex of the occipital region resembled very closely that of the frontal and parietal regions much more closely than it does in man; the first and second cortical layers, as also the deepest stratum, and the adjacent white matter, all agree with the corresponding portions of the human cortex, not only as regards the arrangement of the nerve and other elements, but also apparently with respect to the actual nature and structure of those elements. It is in the relatively greater depth of the third cortical layer—that containing and taken up by the broad band of pyramidal nerve cells—that the brain of the beluga chiefly differs structurally from that of man; in the former there is an absence of any special row of cells fringing the lower margin of this layer, and the large nerve cells referred to were seen in fewer number. But further, in the human cortex there comes immediately beneath the third layer a well-defined band of small closely-set angular nerve cells—a layer which is specially distinct in the occipital region, and which is generally known as the fourth cortical layer; in the cortex of the beluga no such layer has, after prolonged scrutiny, been discernible. Beneath the fourth layer, again, and immediately preceding the deepest stratum, there is in the human cortex a pale separate layer; in the brain of the beluga there is a pale band lying between the row of large cells, which correspond to the third layer in man, and the deepest stratum of the cortex, but whether this is analogous to the pale human layer, Dr. Major states that he cannot pronounce with confidence, and, as this latter layer in man is not recognised by all histologists, he prefers in the present instance to draw no further comparison in respect to it.

Application of Freezing Methods to the Microscopical Examination of the Brain.—In the October number of *Brain*, Mr. Bevan Lewis, of the West Riding Asylum, contributes a paper in which he describes and figures a freezing microtome he has devised, and explains fully the process he adopts therewith, for the preparation and mounting of sections of fresh brain or nerve tissue. The method he has adopted is that by the employment of ether instead of the ice and salt mixture; the instrument consists, in its lower half, of an adaptation of Stirling's microtome—in the upper half, of

a freezing chamber and the section plate. He uses an oval instead of a circular plug; the screw should be $\frac{3}{4}$ of an inch in diameter, finely worked, and with a milled head of at least $1\frac{1}{2}$ inch wide. The freezing chamber should have a false sloping bottom, leading to an exit tube which conducts off the condensed ether. The freezing chamber is made of zinc, is slightly over two inches in diameter, and capped with a plate of the same metal. This cylindrical or drum-like chamber is soldered on the microtome plug, and has three circular openings $\frac{3}{4}$ inch in diameter—one placed in front, the two others laterally opposite each other. The section plate of zinc, $\frac{3}{8}$ inch thick, is placed on the top of a vertical arm, and perforated with a circular aperture, so that the freezing chamber can work freely up and down in it, moved by the screw plug beneath. When about to be used, the freezing chamber is lowered until its upper surface is level with the lower surface of the freezing plate, and a portion of the tissue to be cut, slightly thicker than the section plate, is placed flat upon this upper surface, and a little water is allowed to run round its edges, which subsequently becomes frozen and fixes the tissue firmly to the zinc. The nozzle of the spray instrument is now introduced through the opening in the left hand side of the freezing chamber, and the spray made to play on the lower surface of the top or cap of the latter, on which the tissue has been placed; a cool draught of air should if possible be allowed to play over the surface of the section plate, as it renders freezing still more expeditious. In a few seconds a white band appears at the junction of the tissue and the metal cap on which it lies, and this rapidly spreads upwards until the tissue becomes blanched and frozen throughout. The superficial unfrozen layer is next cut through with one sweep of the section knife, and a smooth surface thus presented from which the fine sections can be taken. Before cutting these, the section knife should on each occasion be dipped into a large vessel of water, and its lower surface pressed on a towel spread over the knee, while its upper surface should be allowed to retain sufficient water to float up the section as it is cut, each section when cut being floated off into a vessel of water; about a dozen sections can be made, after some practice, before it becomes necessary to reapply the spray. In very cold weather a little of the water running off the blade of the knife may become frozen on the section plate; this may be prevented by smearing a little glycerine over the surface of the latter, care being taken not to let it get on the cap of the freezing chamber. After

being cut and floated into a vessel of water, each section is then carefully floated on a glass slide and placed beneath a shade, so as to preserve it from dust, the superfluous water being drained off the slide. The sections are next treated with a few drops of a .1 or .2 per cent. solution of osmic acid, which is allowed to exert its action for ten or fifteen minutes, and then they are again floated off into water to rid them of superfluous acid. When this has been done the sections are transferred to the slides, and stained with a solution of aniline-blue-black—.25 grammes of the powder to 100 cubic centimetres of distilled water—which is either dropped on each individual section on the slide, or all the sections may be stained together in a porcelain capsule or watch glass, half an hour or an hour sufficing to accomplish this thoroughly; the aniline solution before being used being carefully filtered. The sections are next washed, and, if it be desired to mount them permanently, are dried on their slides under a bell glass with a porcelain capsule containing concentrated sulphuric acid, and mounted by dropping on them a little Canada balsam dissolved in benzole, and adjusting the covering glass. The complete success of this process depends in great part on the attention paid to the minute details in the various steps which have been indicated.

The Functions of the Brain.—The following are the conclusions of a lengthy memoir by Luciani and Tamburini in the *Rivista Sperimentale* :—

1. The excitable area of the cerebral cortex presents in dogs and cats, as well as in apes, considerable differences of disposition and boundaries, not only between different animals, but between the two hemispheres of the same animal.

2. We can establish in the dog, by electric excitation, two distinct centres in the sigmoid gyrus for opposed movements for both anterior and posterior limbs.

3. In apes the excitable area for the members and face is not limited solely to the ascending frontal convolution, as Hitzig claims, but extends also to the ascending parietal, the angular gyrus, &c., as Ferrier has shown.

4. The existence of a special epileptogenic zone in a fixed and definite cortical area is inadmissible, but under proper conditions the excitation in any excitable area whatever may cause an epileptic attack, which commonly begins in convulsions limited to the groups of muscles from the centres from which emanates the irritation.

5. The different excitable areas seem variously scattered, probably on account of their being endowed with different degrees of excitability. This may be presumed for many reasons, and especially that to produce a complete epileptic attack acting on the same requires a different intensity of the electric current.

6. It is absolutely impossible that the movements produced by electrification of the cortex can be due to a diffusion of the current to the dura mater.

7. The hypothesis that the movements produced by electrification of the cortical zone may be of reflex nature, in that there are in the excitable area so many sensory centres for the different parts of the body, does not stand against the facts of the decomposition, co-ordination to an end, and constancy of these, since the movements produced reflexly by peripheral irritation do not ever have such characters.

8. The phenomena of lost motility produced by ablation of the excitable zone are of an absolutely paralytic, and not of an ataxic, nature.

9. The aforesaid paralytic phenomena are transitory, but they last the longer—that is, the compensation takes place the more slowly as we ascend in the animal scale.

10. To explain the recovery from the paralytic phenomena the idea is inadmissible of a functional substitution of a neighbouring area, or of the opposite hemisphere, nor can we say that the psychomotor function is located in the basal centres; but it is needful to admit that these last may physiologically be centres of voluntary motion, since the development or perfection of this their function produces the cure of the paralytic phenomena and produces it all the more quickly the more developed this function is normally.

M. Pitres in his *Thèse de Doctorat*, Paris, 1878 (abstracted in *Revue des Sciences Médicales*), considers the identical effects produced by lesions of the subcortical white substance, as by those of the convolutions themselves. He first divides the *centrum ovale* into three zones—(1) the prefrontal or anterior; (2) the occipito-sphenoidal; and (3) the fronto-parietal. This last zone, according to Pitres, is limited by two vertical planes—the one passing within five centimetres of the fissure of Rolando, the other passing one centimetre in front of the internal perpendicular fissure. Situated between the opto-striate bodies and the alleged motor convolutions this median zone of the *centrum ovale* may be subdivided into four regions—(1) the pediculo-frontal; (2) the frontal; (3) the parietal;

and (4) the pediculo-parietal—each corresponding to four parallel vertical planes carried to the foot of the three frontal convolutions, to the frontal, the ascending parietal, and the foot of the parietal lobules. Finally, each of these regions may be considered as itself formed of numerous bundles; thus in the pediculo-frontal we distinguish three—the superior, median, and inferior.

This artificial limitation permits M. Pitres to classify the lesions of the *centrum ovale* by comparing them to the superjacent convolutions; and in a second part he is then able to usefully analyse the numerous clinical observations. He first shows that lesions of the prefrontal zone, like those of the occipital, never cause any disorder, either motor or sensory, at least not directly in the absence of complications. Lesions of the fronto-parietal zone alone have in the majority of cases produced hemiplegic motor disorders, and sometimes also monoplegias. The existence of these monoplegias from injury to the *centrum ovale*, of numerous cases of aphonia from destruction of the left lower pediculo-frontal bundles, of brachial monoplegia from lesion of the middle and upper frontal bundles, &c., prove that the anatomical dissociation made by M. Pitres is true also functionally. The white substance of the centrum, in spite of its apparent homogeneity, is formed of bundles, each adapted to special conduction in the fronto-parietal zone—the sole conductor of movements. The white bundles of fibres remain functionally distinct, like the convolutions from whence they come. In fine, the lesions of the centrum, and those of the superjacent convolutions, produce exactly the same disorders. It is to establish this last conclusion that M. Pitres gives the third part of his memoir. He shows that lesions of the centrum, like those of the fronto-parietal convolutions, are often latent; that the paralysees are often transient and incomplete, or progressive; that they are often preceded or accompanied by contractions, or even by epileptic convulsions; and, finally, they may be followed by slow contracture with secondary degeneration. He even discusses secondarily the pathogeny of primary contractures, that of latent lesions, &c.—(*Am. Journ. of Nerv. and Ment. Dis.*).

Sinew Reflex.—In order to determine whether the muscular contraction due to percussion of a tendon is reflex in its nature or a direct muscular stimulation, G. Burkhardt undertook to measure the time required by the phenomenon. Between the concussion of the tendo-patellaris in man, and the contraction of the upper part of the muscle quadriceps femoris, there elapsed a period of 40-thousandths of a

second, while the contraction of the lower part commenced within 38·7-thousandths of a second. This time is too short to permit a comparison with cutaneous reflexes, which latter consume double that time in passing only through the gray substance of the cord. Similar results were obtained from rabbits. In that case the time consumed was 17·1-thousandth second—that is, one-half to one-fifth of the time requisite to obtain a reflex action from cutaneous stimulation. If, on the other hand, the contraction were due to direct stimulation of the muscle, there ought to be a perceptible difference in the time of contraction of the upper and lower end of the muscle. The velocity of the muscle wave being stated as one to three metres per second, a period of twenty to thirty thousandths second might be expected to elapse while the wave of contraction travelled through the entire muscle of about twenty centimetres in length. In reality, one-tenth of that time elapses between the upper and lower end of the muscle. Furthermore, Burkhardt found—(1) sinew reflexes continue *without* change of duration after section of the spinal roots, or destruction of the lumbar cord, after which, of course, all cutaneous reflexes have ceased; (2) both cutaneous and sinew reflexes continue partly after section of the cord, at the level of the first lumbar vertebra—the time of the former, but not of the latter, is prolonged by this operation; (3) section of the crural nerve prevents cutaneous and sinew reflexes; (4) small doses of strychnia intensify sinew reflexes, but do not change their time; (5) crossed sinew reflexes (from one side to the other) require as much time as cutaneous reflexes. From these data Burkhardt concludes that the sinew reflex is really reflex in its nature, but that it cannot pass through the cord—possibly, however, through the plexus or spinal ganglia. The tendon is hence connected through a nervous route with the muscle, but this route goes neither directly to the muscle, nor does it pass through the cord. The reflex contraction started by stretching of the tendon, as from over-action of the antagonistic muscles, is a process of too rapid onset to be influenced by the will.—(*Centralblatt f. d. med. Wiss.*, 1878, abstracted in *Am. Journ.*)

II.—PATHOLOGY OF THE NERVOUS SYSTEM.

Hemiopia from Cortical Disease.—In No. 21 of the *Centralblatt f. d. med. Wiss.*, 1878, Dr. P. Baumgarten describes a case of left lateral hemiopia coming on suddenly after exposure to cold. The left half of the field of vision of both eyes was completely

gone, the line of separation passing through the fovea centralis, while the other part of the retina, which still possessed visual perception, was normal. This state continued several months without change, until death occurred from cardiac paralysis in renal disease. The autopsy revealed an apoplectic cyst, the size of a walnut, in the substance of the right occipital lobe. Its lower wall was separated from the cavity of the right posterior cornu of the lateral ventricle by a layer of normal medullary substance several millimetres in thickness, while its upper wall was formed by the convolutions of the three occipital gyri, which were in a state of yellow softening, but still recognisable in shape. There was found besides a spot of red softening, of the size of a pea, in the upper wall of the left anterior cornu; also an apoplectic cicatrix of half that size in the centre of the right optic thalamus. No other gross or microscopical lesions existed within the cranial cavity.

Paralysis of one-half of each retina from a lesion of the occipital lobe of the same side, exactly corresponding to this case, was likewise obtained by Munk in his experiments on monkeys.

A case resembling the above is recorded by Dr. Pooley in the *Archives of Ophthalmology and Otology* (Vol. I.) A gentleman, aged fifty-five, suffered from visual hallucinations, epileptiform convulsions, and maniacal attacks. Thirty years ago he had contracted a chancre, and was treated with mercury, no syphilitic symptoms since appearing. The patient was dizzy, mentally deranged, and excited. He possessed a myopia, one-tenth; the visual power was normal. Cerebral syphilis was diagnosed; and iodide of potassium, bromide of potassium, and bromide of ammonium were prescribed. A few days subsequently a right lateral hemiopia of both eyes set in the limit of the field of vision, passing through the centre of the retina. Within some weeks a paresis of the right side of the body and diminished sensibility of the right arm was noticed. Six months subsequently vision was reduced to twenty-fiftieths, and the appearances of choked disc were observed in the left eye. Death subsequently ensued. The autopsy showed the left posterior lobe of the cerebrum enlarged, and in it there was found a yellowish-white, hard, rounded tumour of about three-fourths of an inch in diameter, and one-half an inch in width. The left thalamus and the medullary substance surrounding it was softened. The tumour possessed all the characters of a gumma.

Tendon-reflex in General Paralysis.—Dr. Muhr (*Psych. Centrblt.*,

No. 2, 1878) draws the following conclusions from an examination of fifty-one cases :—

1. The contraction of the quadriceps muscle, on percussion of the tendo-patellaris, occurs almost always in general paralysis, even in advanced cases with paretic lower extremities.

2. In fifty-one cases the tendon-reflex was absent six times. These cases were not at all moribund, or even ataxic.

3. Neither the condition of the pupils, nor any other clinical symptoms, indicate whether the reflex can be produced. The lack of the reflex is found as an isolated phenomenon in different paresis.

From a consideration of Westphal's latest researches, Dr. Muhr finally concludes that an absence of the tendon-reflex in any case of general paralysis is a positive sign of degeneration of the posterior columns, even when no ataxia is observed.

The Cerebral Lesions in General Paralysis.—Mierzejewski, of St. Petersburg, in *Le Progrès Médical*, abstracted in the *An. Méd. Psychol.*, passes in review the opinions of Tigges, Meynert, Hoffman, de Meeremberg, Meschede, Lockhart Clarke, Westphal, Lubimoff, Rutherford, Batty Tuke, and Herbert Major. He arrives at the conclusion that the pathological alterations of the nerve cells described by these authors are not sufficiently pronounced to establish a well-marked distinction between the affected cells and those which are normal, especially as he thinks that certain characters which the authors named regard as pathological, must rightly be looked upon as normal. The rounding of the nucleus which, according to Meynert, is a morbid alteration, he considers to be a physiological condition. The presence of two nuclei in a cell is seen in all irritative processes, and is thus not special to general paralysis. The increase or diminution in the size of the nerve cells has been pointed out by the authors alluded to without any definite indication, or without taking into account the region in which such cells have been found, as the normal size of the cells varies according to the depth of the region examined. Having thus disposed of the observations of the preceding authors, he proceeds to advance the opinion that the principal changes in the ganglion cells are met with round the vessels—there the interstitial basis material (neuroglia) proliferates its elements surrounding the ganglion cells, and which accumulate on the latter. In certain cases the cells thus surrounded become increased in volume (0 mm. .075 to 0 mm. .042); in others they appear shrivelled, and composed of a clouded protoplasm easily coloured with carmine, the neighbouring

healthy cells being coloured with more difficulty. He has never met with a division or multiplication of the nuclei in the nerve cells. In a more advanced stage the cell surrounded by extravasated elements is filled with yellowish-brown molecules, the nucleus being displaced ; the cell appears then filled with a pigment which is not dissolved by ether, and which is but little coloured by carmine. The processes of the cells are also involved in the granular degeneration, but the apical process (Cylindraxe) remains for some time unaffected. In the white substance of the frontal and occipital lobes, M. Mierzejewski found oval bodies forming lines like a knotted ribbon. These are probably hypertrophied axis cylinders, which sometimes break up into such ovoid bodies.

Spastic Tabes Dorsalis (Charcot).—This affection, which must be carefully distinguished from locomotor ataxy, occurs most frequently in males, from thirty to forty years of age ; it is rare, and its etiology is generally unknown. Its progress is slow but progressive, lasting from eight to ten or fifteen years. The symptoms appear to correspond with the existence of a chronic transverse dorsal myelitis with descending sclerosis and consecutive degeneration of the lateral columns. The treatment by hydropathy and the continuous current has not been attended with any definite benefit. Erb, however, has recorded a case where recovery took place under the influence of galvanism.—(*An. Méd. Psychol.*).

Remissions of Dementia in certain cases of General Paralysis.—In the *Annales Méd. Psychol.*, for Jan. 1879, M. Baillarger contributes a paper on this subject, of which the following are the conclusions arrived at as the result of numerous observations :—

1. Melancholia, with paralytic stupor, or simple paralytic stupor of a very grave character, and appearing as an advanced dementia, may, even after many months, be followed by remarkable remissions.

2. When the symptoms of dementia with delusions develop rapidly in the early stages of general paralysis, they often do not point to true dementia, but to a sort of *false dementia*, constituting a special condition not yet sufficiently studied.

3. The existence of dementia at the outset of general paralysis is often erroneously affirmed from certain special characters which, it is supposed, are masked by the maniacal or melancholic delirium.

4. The *restless, absurd, and contradictory* delusions of paralytics is no proof of the existence of dementia, and can be explained by a special state comparable to certain cases of intoxication.

5. The false dementia (pseudo-dementia) in general paralysis may be so far distinguished from the true dementia by its rapid onset, or by the indications of stupor.

6. There are not real remissions in simple paralytic dementia where the course has been slow and progressive.

The Muscular Troubles in General Paralysis of the Insane.—The following are the conclusions arrived at by M. J. Christian in a paper on this subject (*An. Méd. Psychol.*, Jan., 1879) :—

1. There is in general paralysis a true weakening of the muscular force, such as is observed in all chronic affections, though this weakening is often not well marked.

2. There does not exist any constant connexion between the diminution of the muscular force and progress of the marasmus which takes place in this affection. Many months may even intervene during which the marasmus may become greatly increased, and yet the dynamometer will give practically the same results.

3. The affection called general paralysis of the insane is not at any period of its course of a paralytic nature, as up to the end the patient preserves the *will* to contract his muscles, and the *possibility* of contracting them with power.

Glosso-labial Paralysis of Cerebral Origin (Lépine).—Glosso-labial paralysis may have a cerebral origin; the seat of the lesion adjoins that of aphasia. It may be confounded, from its rarity, with glosso-labial paralysis of bulbar origin—from which, however, it is distinguished by the absence of atrophy of the muscles—as well as with bulbar paralysis—*en foyer*—in which the symmetry of the paralysis is less complete. Reflex movements are preserved in glosso-labial paralysis of cerebral origin.

Nocturnal Epilepsy (Memoir by M. Echeverria, *Annales Méd. Psychol.*, March, 1879).—Attacks of nocturnal epilepsy are more frequent amongst women than men, and are associated with vertigo, *petit mal*, or severe attacks occurring during the day—the two former often passing off unperceived. The etiology of nocturnal epilepsy is essentially encephalic, and may perhaps be referred principally to hereditary predisposition, injuries to the head, alcoholism, syphilis, sunstroke, and powerful emotions. Nocturnal incontinence of urine, biting the tongue, petechiæ on the face and neck, are not invariable and constant phenomena, but when they exist, especially the first, they possess an indubitable pathognomonic value. The sudden and instantaneous explosion of a mental excitation during sleep, or in the middle of the night, or the existence of mental derangement

revealed in the morning, are indications of nocturnal epilepsy ; if to this the incontinence of urine is added, and hereditary predisposition to insanity, with eccentricity of character and tendencies to impulsive violence exist in an individual, the diagnosis of epilepsy may be made without hesitation. The immediate onset of the nocturnal attack is not favoured by sleep, which, in general, restores the intellectual equilibrium whenever it terminates the crisis of the *haut mal* or mental excitement. The access of somnambulance never shortens the duration nor the terminal explosion of violence characteristic of the attacks of nocturnal epilepsy, during which the patients speak or execute automatic actions. The nocturnal attacks followed by paralysis are exempt from the immediately consecutive mental troubles. Nocturnal epilepsy causes a complete forgetfulness of anything which may have occurred during the attack, although such event may be remembered by more or less distinct hallucinations or frightful nightmare. Hallucinations of sight predominate amongst nocturnal epileptics, and these are nearly always of a red or fiery colour. The nocturnal epileptic, acting under the influence of an unconscious automatism, is not responsible for criminal acts perpetrated during the persistence of such a condition, but, at the same time, those thus affected are the most dangerous of the insane, and should be placed under surveillance in an asylum.

Systemic Disorders of the Spinal Cord.—Flechsigt, in the second part of a memoir on this subject (*Archiv der Heilkunde*), engages in the demonstration that the secondary degenerations of the cord consecutive to brain lesions have their location in the pyramidal tracts, and that, according to all appearances, only the pyramidal fibres are involved to the exclusion of the foreign fibres intermingled with them.

The anatomico-pathological researches of Türck, Vulpian, and Charcot show that lesions of the gray cortex are not followed by secondary degenerations unless they are situated in the gray matter of the central or neighbouring convolutions. The observations of Türck and Charcot show that the secondary degenerations of the pyramidal fibres are consecutive to lesions of the gray matter of the motor zone and the subjacent white substance. Moreover, the recent researches of Westphal have demonstrated that very superficial lesions of the gray matter, such as are found in progressive general paralysis, rather frequently give rise to secondary degenerations of the pyramidal bundles.

As to lesions involving the gray matter of the central ganglia of

the hemispheres, the observations of Charcot have not as yet supplied the direct proofs that they can exist without giving rise to secondary degenerations. On the contrary, however, Türck has published the account of a case in which a *foyer* the size of a hazelnut existed in the caudate nucleus without any degeneration of the pyramidal fibres. In two other cases of Flechsig's the external zone of the lenticular nucleus was destroyed, and the pyramidal fibres were also intact. On his part Türck has published two cases in which the upper portion of the optic thalamus was altered without secondary pyramidal fibre degenerations. But we still lack direct proofs showing that lesions of the first and second members of the lenticular nucleus, the tails of the caudate nucleus, and the lower posterior portion of the optic thalamus are incapable of producing this secondary degeneration.

Lesions of the internal capsule are those that most frequently give rise to secondary degenerations of the pyramids. Flechsig, however, considers as erroneous the opinion of Charcot that only those lesions that involve the two anterior thirds of the capsule can produce them. Thus, in the two cases of Türck, mentioned above, the secondary degeneration involved the third posterior fourth of the internal capsule, and it is just at this horizon, according to Flechsig, that the pyramidal fibres coming from the cortex traverse the capsule. Moreover, in five observations of secondary degenerations of the pyramids that Flechsig himself reported it was just this part that was involved.

In the cerebral peduncle the study of secondary degenerations is more complicated.

The study of the cerebrum of newly-born individuals shows that the fibres extending from the peduncle to the internal capsule preserve their original relations in the latter.

The fibres which are the most internal in the peduncle are the most anterior in the capsule, and *vice versa*.

Thus are explained the facts observed by Charcot and Vulpian—in case of lesion in the internal capsule the more anterior it is in this tract the nearer the secondary degeneration in the peduncle is to its internal border.

In the medulla and cord the degeneration always invades the pyramidal fibres as they have been described by Flechsig. The extent of the degeneration (as regards the number of fibres involved) depends on the dimension and locality of the primary lesion.

Indeed, at the horizon of the internal capsule the section of the

pyramidal bundle presents a surface fifty times smaller than that of the cortical motor zone in which these fibres terminate. A lesion of the same extent would, therefore, cause the degeneration of a greater number of fibres when it involves the third posterior fourth of the internal capsule than when it occupies the motor zone of the convolutions.

The distance from the side of the cord the degeneration extends in the anterior and lateral columns, and its extent in length, present as many variations as does the distribution of the different bundles of the pyramidal fibres in this portion of their course.

When the degeneration occupies two homologous columns of the cord at the same time, it may be consecutive to a bilateral lesion of the hemispheres, or to a lesion involving the cord itself.

Secondary degenerations of the pyramidal fibres appear to have no reaction on the gray nuclei of the medulla nor on the gray matter of the anterior cornua.

From all this Flechsig concludes that the secondary degenerations of the pyramids constitute rather a lesion of an elementary system of fibres.

In a third article (*Primary Degeneration of the Pyramidal Bundles*) Flechsig undertakes to study the localisation and precise nature of the lesions met with in the spinal disorder, described by Professor Charcot under the name of amyotrophic lateral sclerosis.

I. The lesions of amyotrophic sclerosis are essentially a primary and symmetrical lateral sclerosis of the white lateral columns, a destructive atrophy of a great number of multipolar cells in the anterior horns, and a degeneration of a considerable number of the motor fibres of the anterior roots. But Flechsig endeavours solely to show that these different elements, thus involved, belong all to one system, which represents the most direct route of transmission, connecting the cortical motor centres with the peripheral muscles.

(a.) Thus the lesions involving the white substance of the cord will not be limited to the lateral columns, taken in their *ensemble*, as Charcot would have it. According to Flechsig, of all the cases of amyotrophic lateral sclerosis collected by M. Charcot, only six can be utilised in solving this question of localisation.

But in analysing these six observations the German author comes to the conclusion that the lesion of the white substance always reaches its greatest intensity at the horizon of the pyramidal bundles as he understands them. In the majority of cases one part (the smallest) of the pyramidal bundles passes along the anterior

columns, while the other forms the posterior segment of the lateral columns. These are the places of election of the lesion in amyotrophic lateral sclerosis, only because they usually contain the greater part of the pyramidal fibres. But the anterior columns are likewise involved in the majority of cases. If they are not always involved this is owing to the distribution of the pyramidal fibres in the cord. It may happen indeed that all the pyramidal fibres decussate in the medulla, and pass down in the lateral column of the opposite side; in this case they are lacking in the anterior columns.

It is none the less true that in amyotrophic sclerosis the lesion rather frequently involves that part of the lateral columns that is not composed of the pyramidal fibres. In one case even the posterior columns (more especially the columns of Goll) were invaded. Supporting himself on the preceding facts he criticises the name primary symmetrical lateral sclerosis. This name has the great inconvenience of losing sight of the fact that the only system constantly invaded is the system of the pyramidal fibres which is habitually distributed in the anterior and lateral columns; and on the other hand, the lesion in lateral amyotrophic sclerosis is not truly symmetrical except when the distribution of the pyramidal fibres in the cord is itself symmetrical, which is not always the case. Finally, he does not admit, as yet demonstrated, that the lesion has always the white substance as its starting point.

On these different accounts he proposes to substitute for the term adopted by M. Charcot the name primary fascicular sclerosis of the pyramidal bundles. We must, moreover, distinguish a simple and a complicated form according as the anatomical lesion remains limited to the pyramidal bundles or invades those adjoining.

(b.) The lesion of the gray substance consists in an atrophy with complete degeneration of the multipolar cells, which we find in the anterior horns—that is to say, of the nervous elements that are in direct communication with the pyramidal fibres of the antero-lateral columns. Moreover, the nervous elements, which in the medulla are, in a fashion, the homologues of the great motor cells of the anterior horns, seem to be the only ones there involved in cases of amyotrophic lateral sclerosis.

(c.) In the typical cases of amyotrophic lateral sclerosis the lesion spares the posterior roots, and only invades those fibres of the anterior roots whose axis cylinders are merely the prolongations of the ganglion cells of the anterior horns. It consists in a degeneration

of the nervous elements, the interstitial tissue remaining intact. Moreover, at different horizons, the number of altered fibres appears to be in relation with the corresponding number of degenerated multipolar cells.

In fine, Flechsig comes to the conclusion that in a certain proportion of cases of amyotrophic lateral sclerosis the lesion involves, sometimes exclusively, sometimes in a predominant way, a collection of nervous elements which maintain very intimate functional relations. These elements (pyramidal fibres, multipolar cells of the anterior horns, motor fibres of the anterior roots) constitute what Flechsig calls the direct *cortico-muscular system*—that is, the most direct route of transmission from the gray cortex of the convolutions to the motor cells of the anterior horns, whence originate the motor fibres destined to the striated muscles—in fact, according to the author, the pyramidal fibres extend directly from the peduncles to the cortex, without touching the central ganglia of the hemispheres.

II. In a special chapter, Flechsig discusses lengthily the histological character of the lesion of the white substance of the cord in amyotrophic sclerosis. He is disposed to admit that it is rather a primary degeneration of the nerve fibres, and not a primary hyperplasia of the interstitial connective tissue, with secondary atrophy of the compressed nerve elements. But, in fact, direct proofs derived from microscopic examination are wanting, and Leyden claims that in a proportion of cases of amyotrophic sclerosis the lesion primarily affects the neuroglia, and that atrophy of the nerve elements is only a secondary alteration.

In reality, the differences noticed by Leyden in various cases of amyotrophic sclerosis are, according to Flechsig, not more marked than those which are found in the comparison of a number of cases of secondary degeneration, some recent and others of older date.

Moreover, the systematic distribution of the lesion in the majority of cases of amyotrophic sclerosis is sufficient reason for holding that the primary trouble is in the nerve elements. Indeed, the different systems of fibres which by their union form the columns of the cord are nourished by a single vascular network, and the connective tissue which serves as a support to both vessels and nerve fibres is alike in all. We do not see, therefore, says he, why a lesion having its point of departure in the interstitial connective tissue, and limited to a small portion of the transverse section of the cord, should propagate itself throughout the whole length of the spinal

axis without involving the whole of the white substance.—(*American Journal of Nervous Disease.*)

III. THERAPEUTICS.

Gelsemium in Neuralgia.—Professor Massini, of Basle, recounts his experience of the use of this drug in the treatment of eighty cases of neuralgia of the trigeminus. He prefaces his remarks with a brief description of the physiological action of the drug. Redness of the conjunctiva, pain in the eyelids, contraction of the pupil, double vision, and giddiness, are the symptoms which generally follow the administration of moderate doses. When the dose is increased, slight ptosis, dilatation of the pupil, gaping, languor, and pain in the limbs, are the usual results. The respiration is not affected. In frogs, on the other hand, a large dose produces paralysis of the respiratory muscles, the heart's action remaining unchanged. In cases of neuralgia of the trigeminus Dr. Massini gives twenty minims of the tincture every half-hour up to three doses, and he finds that the first dose usually affords relief, and that the pain rapidly subsides after a second or a third dose has been taken. He has never found it necessary to exceed sixty minims, and only in one case did this quantity produce unpleasant head symptoms. The cases in which the remedy produces most benefit are those of simple rheumatic neuralgia of the alveolar branches of the trigeminus; in those it rarely fails. It also sometimes relieves the pain remaining after the stopping of a carious tooth. Where there is any inflammatory affection of the bone or periosteum no good can be expected from the remedy. The medicine may, if necessary, be repeated several days in succession, the active principle rapidly passing off by the kidneys.

Ergot in the Treatment of Neuralgia.—Marino recommends the hypodermic injection of from 0·15 to 0·25 gr. dissolved in six grains of distilled water. This may be repeated once or twice, though perhaps not more than six times, and acted well in certain forms of neuralgic pains, especially in tic douloureux. It appears to act less favourably in sciatica.

Treatment of Hysterical Reflex Neuroses.—Professor Weber recommends the protracted employment of chloroform inhalations in the treatment of obstinate and severe cases of hysterical reflex neuroses of the respiratory apparatus when the primary seat of irritation cannot be discovered and treated. He has himself proved the value of the inhalation in several cases. His first case was of a

lady with a spasmodic cough that had proved rebellious to all treatment. She was cured in eight days by the chloroform inhalations, which were administered as often as the cough came on. A child with sneezing spasms was cured in three days by the chloroform. Another lady with a spasmodic cough was treated with the same remedy for fourteen days, the inhalations being at first administered four or five times, and afterwards two or three times daily. She was much improved; the cough only came on after a walk, and the inhalations were only required then. In four weeks she was discharged cured; a subsequent relapse was cut short by the internal administration of chloroform. In the case of a girl fourteen years of age, who suffered from spasms of sneezing, the inhalations produced a speedy cure.—(*Am. Journal.*)

Ether in Sciatica.—Dr. C. G. Comegys recommends the hypodermic use of sulphuric ether in sciatica. He mentions two cases, one in detail, of this affection cured by this treatment. The injections—of thirty drops repeated twice at an interval of twelve hours—were superficial, not deep, and, though causing severe pain for a time, left no ill effects. He thinks one dose sufficient for a cure in one of his cases, and believes that the remedy will be equally effective in tic douloureux.—(*Cin. Lancet and Clinic*, Jan. 4, 1879.)

Cannabis Indica in Epilepsy.—Dr. Wharton Sinkler (*Phil. Med. Times*) reports having used cannabis indica in two cases with advantage, one of which is given in detail:—A boy ten years of age suffered from epilepsy, which commenced as *petit mal*, occurring once or twice a day for two months, then changing to (chiefly) *grand mal*, occurring about every two hours at night. He was treated with bromide of potassium and tincture of the chloride of iron, which, however, only modified his attacks but gave no permanent relief; as soon as the medicine was stopped the attacks were more severe than ever. Then oxide of zinc was tried, but no good effect was produced. This was discontinued and cannabis indica, one-sixth of a grain three times a day, was ordered, with the result of first decreasing the number of his attacks to one a day for a week, and then stopping them altogether. At the date of the report there had been none for nearly three months. The child's intelligence and disposition, which had before been much injured by the disease, were also wonderfully improved. A second case was also improved, but the full effects of the drug were not yet tested on it.

THE MEDICAL
SOCIETY OF THE
COLLEGE OF PHYSICIANS
OBSERVATION

PART IV.
MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE
COLLEGE OF PHYSICIANS.

SESSION 1878-9.

HENRY H. HEAD, M.D., President.

GEORGE F. DUFFEY, M.D., Honorary Secretary.

Wednesday, April 2, 1879.

S. M. MACSWINEY, M.D., in the Chair.

Post Mortem appearances found in a Case of Cyanosis. By CHRISTOPHER J. NIXON, M.B., Univ. Dubl.; F.K.Q.C.P.; Physician to the Mater Misericordiæ Hospital, &c.

IN April, 1878, I exhibited to the Society a boy who was the subject of well-marked cyanosis, presumably due to congenital malformation of the heart. As I had an opportunity of tracing the case to its termination, I think it right to bring before the Society the history of its progress and its result.

IN May, last year, the boy unfortunately contracted varioloid, and was admitted into the smallpox ward of the Mater Misericordiæ Hospital. Some two or three weeks after his recovery from this illness the fainting seizures to which he had previously been subject recurred with gradually increasing frequency; he became so weak as to be unable to remain up, and, when in bed for some days, a pyrexial condition developed itself. During the continued fever from which the boy suffered, evidences of congestion of the posterior parts of the lungs manifested themselves. Soon afterwards he was attacked by severe general convulsions with loss of consciousness. In the intervals between the convulsions the patient was drowsy and in a state of stupor. He bled frequently from the nose, mouth, and ears; dark spots of ecchymosis appeared upon the legs; the cyanosis became more and more intensified,

and, during a severe and prolonged convulsive seizure, the boy died, in the latter part of June, 1878.

The *post mortem* examination of the thoracic viscera revealed the following results:—The heart is considerably enlarged; it weighs $8\frac{1}{2}$ oz. and is somewhat square in shape, there being no distinct apex. The conus arteriosus forms a remarkable prominence, which is abruptly constricted by the root of the pulmonary artery. This prominence gives the appearance of great enlargement of the right ventricle. Examining the different cavities in order, it is found that the right auricle is very much dilated and that its walls are hypertrophied; the auricular appendix is stunted in appearance, but it shares in the thickening of the walls of the auricle. The fossa ovalis is unusually large; it is minutely cribriform at its upper part, whilst at its anterior margin a longitudinal, oval slit, with finely bevelled margins, exists. The slit can only be observed by traction of the septum in a transverse direction, otherwise it is so completely closed that it would seem not to have been traversed by a current of blood. The opening of the superior cava is of normal size, whilst that of the inferior cava is greatly enlarged and deficient in any trace of a Eustachian valve. The opening of the coronary vein is of normal size, and it is guarded by a fairly-developed valve of Thebesius. The cavity of the right ventricle is greatly enlarged. It presents a very singular appearance from the number of openings in connexion with it and from the hypertrophied condition of the *carneæ columnæ* and *musculi papillares*. The auriculo-ventricular opening is of normal size. The column of King and the moderator band are remarkably hypertrophied. The direct continuation upwards of the ventricle ends in the aorta, which, owing to a deficiency in the upper part of the septum *ventriculorum* may be seen to spring also from the left ventricle. The septum is very much thickened below, gradually becoming thinner towards its upper part, where it is smooth and wedge-shaped. A circular aperture, with an even, thin margin, admitting of a second communication between the two ventricles, exists at the upper and back part of the septum; it admits with ease the tip of the little finger. What may be described as a supernumerary ventricle is found springing from the upper part of the great cavity. A circular opening, admitting with ease the extremity of the thumb, leads into a cavity about the size of a walnut. This cavity is constricted somewhat at its middle, and posteriorly, by a transverse ridge, partly muscular and partly membranous; the upper portion is rough from the presence in it of *carneæ columnæ*, the endocardium presenting in parts a white, rugose appearance, from fibroid degenerative change. Above, the cavity ends in front in a muscular *cul de sac*—the conus arteriosus—whilst posteriorly it leads to the root of the pulmonary artery. The orifice of the artery is found to be completely closed by the intimate union of the margins of the semi-

lunar valves. There are only two pulmonary valves, one being situated to the right and the other to the left side. The segments are perfectly smooth and form a dome, the convex portion of which projects into the artery. In the centre of the dome there is a deposition of rough lymph, which in parts has undergone calcareous degeneration. No communication existed between the artery and the ventricle until a small probe was passed with some force through the fibrinous plug occupying the centre of the diaphragmatic valve. If there have not been complete atresia of the pulmonary artery during life, the opening could not have admitted more than an ordinary-sized pin through it. Beyond the ostium the pulmonary artery is reduced in size, but its primary divisions are apparently of nearly their normal calibre. The coats of the vessel are greatly thinned. The ductus arteriosus is a fibrous cord; it is perfectly closed.

The left side of the heart forms apparently about a fourth of the entire organ. The cavity of the left auricle is small—that of the auricular appendix especially so—and the four pulmonary veins are about one-half their usual size. The left ventricle is greatly diminished in size, and its walls are much thinner than those of the right ventricle, in the proportion of about 2 to 3. The aorta is displaced to the right side, or rather the left ventricle is pushed to the left by the hypertrophied and dilated right ventricle, so that the aorta seems to spring directly from the latter. The aortic semilunar valves are normal in appearance and number. Two of the segments, when turned upwards, cover the mouths of the coronary arteries. The aorta diminishes unusually rapidly in size, the middle portion of the thoracic aorta admitting barely the tip of the little finger. The origins of the great vessels from the arch are peculiar in being quite close to each other. The lungs are small; they weigh 18 oz. Both were consolidated at their posterior portions. The right lung has only two lobes. At the roots of the lungs the pulmonary veins are found to be small in size, the pulmonary and bronchial arteries are comparatively large. Looking at the specimens it would seem that the afferent pulmonary circulation was carried on altogether by the bronchial arteries. I am unable to say whether there existed any collateral circulation through branches of the subclavian arteries, or through the œsophageal arteries, as has been observed in cases of cardiac malformation similar to the one brought by me before the Society.

I do not propose dwelling at any length upon the etiology of the condition of the heart exhibited. It represents a form of malformation which has been very fully described by Rokitansky, Dittrich, Peacock, and others; and a very comprehensive account of the different abnormalities met with is given by Lebert in the 6th volume of Von Ziemssen's *Cyclopædia of Medicine*. I may, however, mention that two conditions very generally coexist in congenital malformations of the heart—evidences of foetal myocarditis and arrest of development. The callous appearance

of the endocardium in the conus arteriosus, and the atresia of the pulmonary artery, illustrate, in my case, the former; whilst the arrest of development is shown by the patulous condition of the septum ventriculorum, the existence of but two pulmonary valves, and by the size of the lungs. The endocarditis and subsequent stenosis of the conus, with what seems to have been a complete atresia of the pulmonary artery, must have occurred prior to the closure of the septum—that is, before the third month of foetal life. The preference of foetal myocarditis to the right side of the heart is very plausibly attributed by Peacock to the transitory interruptions of the circulation which may occur in the umbilical arteries and in the placenta, just as in after-life changes of pressure in the arterial system may give rise to disease of the origin of the aorta. Moreover, according to Friedreich, during foetal life there exists much greater pressure upon the valves of the right side of the heart than upon those of the left.

With regard to the cyanosis, it is interesting to note that it did not develop itself until three years before the boy came under observation in hospital. Obviously it cannot be attributed to an admixture of the two kinds of blood—a point urged by Cloquet, Gintrac, and others. Cases are on record where there has been a patent foramen ovale permitting a free admixture of the two kinds of blood without the occurrence of cyanosis. It would seem to be due, as Louis pointed out, to a blood-stasis in the capillary circulation. Its development after some years, as in my case, must be owing to a defect of compensation arising from some increased demand upon the circulation, either for the processes of growth and nutrition, or for the maintenance of some inflammatory or pyrexial condition which has been set up.

There is only one other point to which I desire to allude—the absence in this case of any evidence of advanced disease of the lungs. Writers on malformations of the heart allude to the frequent association of caseous pneumonia with atresia or stenosis of the pulmonary artery; and Lebert discusses, in his article on this subject, the dependence of pulmonary tuberculosis on pulmonary atresia. I can only say that the specimens which I have the honour of exhibiting do not sustain the views of these observers.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

FORTY-FIRST ANNUAL SESSION.

EDWARD B. SINCLAIR, M.D., President.
WILLIAM ROE, M.D., Honorary Secretary.

Saturday, April 5, 1879.

E. B. SINCLAIR, M.D., President, in the Chair.

A Complicated Midwifery Case. By DR. M'CLINTOCK.

THE following history possesses interest chiefly on the ground of so many deviations from ordinary childbed having occurred in the same patient and on the same occasion:—

The subject of the case was a lady aged forty-five, of rather tall, spare make, who fell in labour of her *first* child the 21st January, two years after the date of her marriage. She had been troubled for some days previously with spurious pains, which had caused her much annoyance and loss of rest. The accession of true labour took place about three weeks before the full term. The first stage proceeded very slowly, notwithstanding the use of the warm hip-bath, of chloral, and of chloroform inhalation. At the end of thirty hours from the commencement of actual labour the patient's condition was as follows:—Her pulse varied from 116 to 120; she was hot, irritable, and much fatigued; the pains were feeble and short; the os uteri was only two-thirds dilated, so that its entire circumference was easily felt, whilst, in the absence of pain, it was tolerably thin and yielding to the finger; the head was partially in the pelvis, but, of course, covered in great measure by the expanded cervix, and lay in the fourth position, the anterior fontanelle being directed to the right acetabulum. In the absence of uterine contraction I could touch the left ear behind the symphysis. There was some abdominal tympany, and the vaginal discharge was scanty and somewhat discoloured. On a careful review of her symptoms and state it appeared very desirable to terminate the labour; and the head being in the pelvis, and the os uteri free from any decided rigidity, the employment of the forceps seemed justifiable. In the first instance a dose of ergot was given; next chloroform was given at her own urgent request; and then the blades of Beatty's forceps (the same one I have been using for over twenty years) were cautiously and gently applied over the ears of the foetus, in true Smellie fashion, the

tip of each blade resting on a cheek. The extraction of the head was conducted very slowly, and I was agreeably surprised at the ease with which it descended, and the readiness with which the os yielded to the dilating force thus applied to it. I should mention that the pelvis was well formed and the child a male, but small. The os did not show any tendency to prolapse with the descending head, which, having been changed into the first position, cleared the outlet in the course of fifteen minutes without any injury whatever to the perinæum. The child was alive, but feeble. There was no trace on its head or face left by the instrument.

When compressing the belly, immediately after the separation of the foetus, I detected the presence of two or three sub-peritoneal fibroid tumours (each about the size of a large Spanish chestnut) on the anterior and lateral surfaces of the uterus. The presence of these growths seemed in some degree to explain the inert action of the uterus, and made me apprehensive of further trouble in this (third) stage of the labour. These apprehensions were very soon realised, for hæmorrhage set in almost immediately upon the birth of the child. After waiting for about fifteen or twenty minutes, and diligently using means to stay the loss and to get away the afterbirth, but without any prospect of success, I proceeded to the manual extraction of the placenta. This was found to be somewhat difficult of accomplishment, as there existed a circular contraction of the uterus some distance above the os, which contraction offered very considerable resistance to the passage of the hand, though she had inhaled some chloroform at the outset of the operation. The placenta was found to be closely—indeed morbidly—adherent to the front upper part of the womb; its texture, too, seemed unusually soft and friable, so that in the attempts to get it away it broke down completely, and the hand had to be twice re-introduced to collect and remove fragments; even so, I was conscious that a portion remained behind adhering to the uterus.

She now got some ergot and brandy by the mouth, and 25 minims of the fluid extract of ergot hypodermically. By these measures all hæmorrhage was completely arrested, and she gradually recovered from the state of weakness to which she had been reduced.

To counteract the effect of any septic discharge, the vagina was syringed twice a day with permanganate of potash or carbolic acid, properly diluted. The lochia were rather abundant and pale, but devoid of foetor. For nine days matters went on most satisfactorily—no rigor, no fever, no pain, no tenderness, and a good secretion of milk. On the afternoon (about 3 o'clock) of the tenth day, however, she was rather suddenly attacked, whilst lying in bed—from which posture she had not yet moved—with a very profuse hæmorrhage from the uterus. This was with difficulty controlled by the use of cold, ergot, and styptics, but broke out afresh some hours afterwards to a more alarming extent. In point of size the uterus

at this time was scarcely perceptible behind the pubes, and the os uteri was so far closed as to offer resistance to the introduction of a finger. A small fragment of placenta was found at the os and was easily removed; it was not in any way offensive. The patient, as may easily be supposed, was very much weakened by these losses, and it appeared highly important to prevent, if possible, any recurrence of bleeding—at all events until her strength could be recruited. Having first thoroughly syringed out the vagina with a weak carbolic acid solution, I introduced a large speculum; a plug of absorbent cotton, thoroughly saturated with solution of perchloride of iron, was then pressed into the os uteri, and followed by a tampon of cotton which closely filled the vagina. This done, 3 grains of ergotine were injected into the buttock. The stuffing of the vagina and os uteri was allowed to remain *in situ* for thirty-six hours, during which time not one drop of blood was lost, nor after its removal was there any recurrence of hæmorrhage, so that her subsequent progress to complete recovery was rapid and unbroken.

I shall now, Sir, with your permission, briefly review the points of special interest in the foregoing history, and submit a few remarks upon each as we proceed.

1. The first point, then, worthy of notice was the *age* of the patient. She was in her forty-sixth year. To meet with a *first* labour at so advanced an age is extremely rare. Such never before occurred to me in private practice. Last year I delivered a lady aged forty-three of her first child, and even to that I do not remember any parallel in my experience among patients of the same rank of life. In Dr. Collins' Report of the Rotunda Hospital, 28 women are stated to have been 45 years or upwards, but the number of labours each of these women had can only be made out in *five* instances, and not one of these was a primipara. On looking over the tables in Drs. Johnston and Sinclair's Report of same hospital, I find the entries of *ten* patients whose ages were 45 years or upwards, and of these only 2 were delivered of first children—1 aged 45 and 1 aged 47. During my own Mastership of same institution, there were 18 women who gave their ages at 45 or beyond it, and out of the entire number only *one* (aged 46) was a primipara. Dr. Matthews Duncan has investigated this matter with his usual care and ability, and his tables show that of 185 women marrying after 40, only *twelve* produced first children.

2. The next point worthy of notice in the above clinical history was the imperfect dilatation of the mouth of the womb, in spite of *tincture of time* and various relaxants. Three causes co-operated to produce this unfavourable condition—viz., (1) early escape of the liquor amnii; (2) the head occupying a fronto-cotyloid position; and (3) feebleness of the uterine action—the first of these causes prejudicially influencing the incipient dilatation of the os, and the last two causes retarding its complete dilatation. Multiplied experience has shown me that the second

of these causes (*i.e.*, an occipito-posterior position of the head) is very influential in retarding the dilatation of the os, especially if the membranes be broken; and this holds good not alone in first but even in subsequent labours. Where this cause of delay is in operation it is of some importance to be able to recognise it, and abstain from measures that can do no possible good.

3. In determining upon the use of the forceps when the os uteri was still little more than half dilated, the reasons which swayed me were these:—The general state of the patient plainly showed the necessity for a speedy termination of the labour, out of regard both for the child as well as the mother; and the local conditions did not appear to preclude the easy application of the forceps. In fact, this was a most favourable case for carrying out Dr. Johnston's practice; and, influenced by his example and success, I had no hesitation in employing the instrument, with due regard to all the precautions enjoined by him. I frankly confess that, had I obeyed the obstetric principles taught me at the outset of my professional life, I would not have entertained the idea; and I am bound to express my sense of obligation to Dr. Johnston for pointing out, as no one had previously done, the safety and advantage with which the forceps may occasionally be used under these exceptional circumstances. The man who first deduces and lays down a broad general rule does eminent service to his profession or art. Again, he who points out and defines a large class of exceptions to the rule is a benefactor. And, lastly, he who by care and penetration discriminates between individual exceptions, and demonstrates which of these may properly and advantageously be brought under the operation of the general rule—this man also advances our knowledge and extends the utility of our art. And this, in my humble judgment, is precisely what Dr. George Johnston has done.

The foregoing case is the second within the past year in which I deliberately imitated Dr. Johnston's practice. The other case—also a primipara of advanced age—gave much trouble, as the head was high up, just entering the brim; and, though the amount of orificial dilatation was a trifle more, still the circle of os could be easily felt all round. Dr. Atthill saw this case with me, and we applied the long double-curved forceps. Immense force was required to extract the head, but the mother sustained no injury, and made an uninterruptedly good recovery. The child, however, was not saved. Four years ago I had a case exactly the counterpart to this, which Dr. Johnston saw with me. Barnes' forceps was applied by Dr. Johnston, and a living male child extracted, which, with its mother, did well. These three cases constitute the amount of my experience of the use of the forceps with an imperfectly-dilated os uteri.

4. Although the presence of one or more sub-peritoneal fibroid tumours in the uterus is not likely to interfere directly with the course of labour, still I think that where there are two or more of them, as in the instance

before us, they may have some influence in causing the uterine contractions to be irregular, or to be inert, as they were all through this case.

5. The constriction of the uterus, which offered so great an obstacle to the passage of the hand up to the fundus, had its seat most probably in the upper part of the inner os, though at the moment it appeared to be higher than this. Had the hand been introduced sooner, this stricture would have offered less resistance, or, perhaps, would not have existed at all. I have always held the opinion that the earlier the hand is introduced after delivery, the easier and less dangerous will the operation prove. This conviction has, to a certain extent, influenced my practice, and, in consequence, I may have resorted to the operation somewhat oftener than other accoucheurs; nevertheless, in thirty-five years' private practice I have never lost a patient who had undergone this manual extraction of the afterbirth. Do not, I pray you, let anyone imagine, from this statement, that I would make light of the operation, as regards either its performance or its consequences. Quite the contrary; it is one both difficult and dangerous, and I never undertake it without extreme reluctance and a painful sense of responsibility.

6. Although septicæmia was much to be apprehended in this case, yet no symptom of such appeared throughout its whole course; and this exemption I would attribute to the daily and careful use of antiseptic vaginal injections, and to the total absence of any perinæal laceration.

7. The secondary hæmorrhage, you will remark, did not take place till the tenth day. When a small portion of the placenta remains *in utero*, as occurred in the case before us, hæmorrhage seldom comes on till the fifth or sixth day, and oftener not till after the ninth day.

About three years ago I saw, in consultation, a very bad case of secondary hæmorrhage which had been going on for days, and had contributed to reduce the patient to a state of extreme weakness. Here I removed a bit of the placenta nearly three weeks after delivery, and all further loss of blood ceased. When I first saw this patient she had well-marked symptoms of pyæmia; subsequently a large abscess formed on the forearm, and she got severe phlegmasia dolens of one leg. She survived these accumulated troubles, however, and eventually regained perfect health.

The PRESIDENT said they had heard this interesting case brought forward by Dr. M'Clintock in his usual able and perspicuous manner. There were a great many points raised in it of particular interest to the members of the Society, and he (the President) would be happy to hear any expressions of opinion that those present might wish to give.

DR. DOYLE said that a case in which the forceps had to be used before the dilatation of the os uteri came under his observation. The woman was in a dying condition, and it was used in order to save the child's life.

She was pulseless and her extremities were cold; the os was undilated and rigid, and altogether the case was not a favourable one for the employment of the instrument; but, as nothing else could be done, it was determined to use it. He accordingly introduced it, and succeeded in delivering the woman. The child lived, but the woman died. However, he considered that in this case, in order to save the life of the child, he was justified in acting as he did.

DR. MORE MADDEN.—As to the question of the use of the forceps before the os uteri was fully dilated, there could be no doubt that in a case where the pulse of the patient was from 116 to 120, and when she was feeble and worn out he (Dr. M'Clintock) was wise in employing it. Under such circumstances the propriety of using the forceps could not be disputed. In a case such as Dr. Doyle had detailed he (Dr. Madden) would first have dilated the os with the fingers to a size sufficiently large to admit the forceps. However, he would prefer to employ version rather than use the forceps. He had a case in which a lady had married late in life, and he thought that she had come to the change in life. She was forty-five years of age, and he distrusted the idea of pregnancy when she actually became so. She had only been one hour and a half in labour when she was delivered of a healthy child.

DR. CRANNY would wish that some of their obstetricians, before condemning this practice, gave it a fair trial in proper and suitable cases. They knew many of the profession who cry out that it was wrong; sure to do mischief; was a great risk to the mother; and did not save the child. There was no doubt that an inexperienced practitioner might employ the forceps in an unsuitable case, and so do harm, but if the case were properly selected there was never any harm done. He (Dr. Cranny) had seen many cases treated by Dr. Johnston, in the Rotunda Lying-in Hospital, Dublin, and had himself operated with that gentleman's sanction, and he never saw one where harm was done by introducing the forceps before the os was fully dilated, but of course the cases were suitable.

DR. DENHAM fully concurred in the possibility and propriety of inserting the forceps and effecting delivery before the os was fully dilated. It requires an experienced hand, and no young man should rashly attempt it, especially in a case where the patient had fibroid tumours in the uterus. A case in point he had under his own observation. A poor woman, who resided in Russell-street, had married late in life. When he saw her she was dead. It appeared that she had been attended by a midwife, and, owing to the hæmorrhage that set in, she was sent to hospital.

He also thought that in the case of retained placenta the early introduction of the hand was good practice. Many women would be saved by removing the placenta with the hand, and not waiting for the dilatation of the os. A small portion may be permitted to remain, which will ultimately cause death.

DR. JOHNSTON concurred in what Dr. M'Clintock and Dr. Denham had said with reference to the retention of the placenta—that is to say, that when once the hand had been inserted every particle should be removed. In a case in which a small portion of membrane was so morbidly adherent that it may require time and trouble to take it away, the doctor should not withdraw the hand from the uterus till every piece should be removed, and till there should no longer be any danger of secondary hæmorrhage. The employment of injections of perchloride of iron in cases of hæmorrhage was useful, but the operation should be performed cautiously, and the fluid thrown slowly and carefully over the surface.

The PRESIDENT said he should express his most unqualified satisfaction that the practice of his old friend and colleague, Dr. Johnston, in inserting the forceps before the os uteri was dilated was coming more into favour. He (Dr. Johnston) had successfully done so when the os was less than two-fifths open. On a former occasion Dr. Johnston was one of the first that broke the neck of the practice that a “forceps was not to be used on a primiparous woman,” and from further experience he can now say that he can use it when the os is not so much as a third dilated. Dr. M'Clintock was one of the first to prove in this country that Dr. Johnston's practice was right, and he (the President) would not hesitate to follow out his principles. As to the introduction of the hand to remove the placenta, he had long followed out the principle of the immediate introduction of the hand. He had had many cases in Sir Patrick Dun's Maternity, in which he had to do so, and he never had a bad case when he followed the principles laid down by Dr. M'Clintock. He agreed in Dr. Denham's remarks as to the treatment of placenta prævia. Dr. Lee, of London, used to act in the manner mentioned. He used to introduce the whole hand to the vagina, separate the placenta at one side, dilate the os, catch the foot of the child, and then deliver cautiously. He should say, in conclusion, that the paper was as interesting as any that had ever been read before the Society.

Notes on Diarrhœa in Children. By HENRY KENNEDY, A.B., M.B., F.K.Q.C.P.I.; Physician to Simpson's Hospital, and the Whitworth, Drumcondra.

THE subject I would bring before the meeting this evening has been often discussed, and all writers on children's diseases have special chapters on diarrhœa in its various forms. Many of these are very elaborate, and, if I may so say, the treatment is so likewise, and yet, after reading one of them, the problem still remains—what is the best line of treatment to pursue, or the proper medicine to administer? I presume I address few who have not found their best efforts foiled—at least in some cases. I know this has been my own experience, extending now over a good many years—sometimes thinking I had found a remedy which was as certain as medicine admits of, but on further experience learning that it also might fail. It is on account of this very uncertainty I have been induced to bring the subject under notice this evening; nor has it been unintentionally that I have given the name of "Notes" to what follows, for this will enable me to select such points only as seem to me worthy of notice, and besides, to have attempted to enter into the subject at large would have been as wearying as it would be useless to the meeting I have now the honour of addressing.

I begin by making a very obvious remark—I mean the very important place affections of the gastro-intestinal mucous membrane occupy in the diseases of childhood, and this whether arising from sympathetic affections, or from actual disease. No doubt of it the sympathies exist all through life; but in the young they are of a far more exquisite kind, and growth being the great leading characteristic, and digestion the means of accomplishing it, Nature has bound these together by the closest ties, and there are few, indeed, of the illnesses of childhood which do not exhibit more or less derangement of the intestinal tract. I need scarcely remind you of the ease, comparatively speaking, with which convulsions may arise at this period of life, or of the vomiting which occurs in hydrocephalus, or the diarrhœa which is so constant an attendant on teething. In all the specific fevers of the young the same is to be observed. The worst cases of scarlatina are all ushered in by vomiting and purging. In measles, too, I have seen a most formidable complication in the form of acute dysentery, and this also may be seen, though more rarely, in whooping-cough. In typhoid, too, diarrhœa is present; but here it may be said to exist as a matter of course. Lastly, we have diarrhœa of varied kinds, constituting in itself a most serious affection, and swelling at times the bills of mortality to a great extent, and, if I mistake not, much more prevalent in London than with ourselves—making allowance, of course, for the difference in the population. Now it is of this diarrhœa I would speak here, in a few rough notes.

Possibly a word about its morbid anatomy may not be out of place, as a commencement of these notes.

The small amount of actual disease found in fatal cases has always appeared to me remarkable. Even when the symptoms would lead one to expect disease, little or nothing is found. Paleness of the mucous membrane, and apparent thinning of the coats of the intestines, are what commonly exist, and the mesenteric glands do not seem to participate in the affection. In exceptional cases, however, signs of inflammation may be detected in the form of effusion of lymph, but in very small patches, as it were, and with surrounding redness. These patches may be readily overlooked, owing to their small extent, but they are not the less important to recognise; for I believe, if we knew of their existence, our treatment should be modified accordingly, of which I shall say more further on. In other cases again, though still more exceptionally, lymph is poured out to a much greater extent. This was the case in the epidemic of measles of which I have spoken, and here there was also extensive ulceration, constituting, in fact, dysentery, and affecting mainly the colon. I have seen instances, however, where lymph existed without any ulceration, and in one most remarkable case there was found a coating of lymph which literally reached from the stomach to the rectum. This child had the most obstinate diarrhoea, but no symptom which pointed to the state I have described.

When the diarrhoea is very chronic, it causes, or seems to cause, a state of the mucous membrane which is worthy of note, and chiefly affecting the colon—I mean a pulpy and swollen state of the membrane, the swelling being apparently due to the effusion of serum. I am not sure but that this state has been described, though I cannot say where I have seen it.

If such be the state of the mucous intestinal tract in the diarrhoea of children, it certainly is not easy to explain the obstinate character of the disease which it so often presents; and, for myself, I have come to the conclusion that there must be some constitutional peculiarities to account for it. Of these, probably, by far the most frequent is the strumous diathesis, which, there can be no doubt, prevails to an extent that it may be questioned whether we even yet sufficiently recognise. I recollect the late Sir Henry Marsh making a similar remark; and that many cases are so tainted, without exhibiting, in any marked degree, the outward signs of this state, is certain. In another place I have advanced the opinion that typhoid fever occurs chiefly in persons of the strumous diathesis; and, unless I am much mistaken, I would say the same of the diarrhoea of children—I mean the majority of cases. This I know, that what we understand as constitutional treatment—such, for instance, as change of air—will often succeed in effecting a cure where all our drugs and other treatment have signally failed; and this, I consider, puts

in a very strong light the opinion I have advanced, that in treating the diarrhoea of children it is often a constitutional affection with which we have to contend.

In close connexion with the strumous diathesis, and yet not the same, is another state which calls for notice here—I mean rickets, or a tendency to this disease. In my experience the most obstinate cases of diarrhoea occur in such constitutions, and when called on to treat these cases we should be very particular in our inquiries on this point; and I may add, in passing, that it is for this purpose a very good plan to have the child entirely stripped. This means often reveals what would otherwise escape our notice.

Amongst the remote causes of diarrhoea in children we must not omit the epidemic tendency, which is, at times, so marked and so very general. I believe it is very much better to avow our entire ignorance of the cause of this than to have recourse to such causes as bad sewerage, or bad water or milk. It is enough to say that whilst these latter are always more or less present the epidemic tendency is not so.

Of the special causes of diarrhoea in children I need scarcely speak. Allusion has been already made to the teething, and the strange sympathy which exists between this process and the mucous membrane of the intestinal canal. I shall only observe of it here that I believe this form of diarrhoea is very generally, if not always, a salutary process, and more to be encouraged than checked.

A much more general and direct cause of the complaint is the food; but on this point the standard works of the day give such accurate directions that little or nothing remains to be said on it. There are two points, however, which I must notice. The first is the habit which prevails at the present day so very generally of feeding the infant both by the mother, or wet nurse, and at the same time by artificial feeding, as it is called, or by the latter alone. It seems to me there must be a constant risk in either of these plans. Even allowing the cow's milk to be pure, and all the points connected with cleanliness, &c., to have been attended to, I still think there is risk; and this I know, that in nearly all the cases of diarrhoea I have seen of late years it has been this compound way of feeding infants which seemed to have caused the disease. I believe the profession, as a whole, object to it, and yet it has become most general. That it is contrary to Nature's teachings is certain, and should not be put in force—at least till the teeth have made their appearance. It may be said that many children are so reared, and reared healthy; but I think this is owing to the children being naturally strong, and so able to resist the bad effects of this mode of feeding.*

* I would here notice a recommendation of Dr. Faussett, of Clontarf, which consists in the use of cocoa for infants that must be artificially fed. The suggestion is a valuable one, and one which I have now had carried out in different cases with good results.

The second point, relating to the food, I would put in the form of a query. Are infants or children ever overfed? I believe they are, and that both mothers and nurses are very apt to overdo the thing. The desire is so great to make the child strong that the mistake is very apt to occur; and this may show itself in another way—that is, by the food being given too frequently. Not that young children are not to be frequently fed, but that this point, like every other, may be overdone, and so the stomach never get the necessary rest.

And now a few remarks as to medical treatment; and though I have no new medicine to propose, I believe some have fallen into disuse that ought not to have been forgotten, and foremost amongst these I would put calomel, which, for some reason or another, is by most now looked on as a dangerous medicine, and not to be used. I believe, however, that with common discretion it may be as safely used now as it was by Clarke some eighty years ago. In my own experience it seems to act best in infants where the diarrhoea has only been of short duration, and is frequently attended by those greenish dejections which are so characteristic, and to which Dr. MacSwiney has, not long since, called attention in an interesting paper read before this Society. I have the impression that when greenish stools exist there is a good deal of irritation, if not inflammation, present,* and that this may be some explanation of the drug acting so favourably; but, whether or not, it is certain that the majority of infants bear calomel wonderfully well. Exceptions, I know, there are, but this applies to all our drugs. Clarke used to give the calomel in half-grain or grain doses, rubbed up with a little sugar—and a very good way it is. In some old book or other I saw it recommended to mix the calomel with some chalk, and a most useful combination I have found it, and still more powerful than the calomel by itself. How the calomel acts I shall not take on me to determine, for, now-a-days, we have been told by Bennet and others that it is not on the liver its influence is felt, but on the intestinal tract. I confess it seems hard to accept this teaching. It is scarcely necessary to say that the giving of calomel need not interfere with the use of other drugs at the same time.

A second medicine which has fallen into disuse for the treatment of diarrhoea in children is “hippo” (ipecacuanha), and yet it was once, about a century since, in great repute, and I have found it myself singularly useful. Indeed as a single remedy I know none equal to it; and as I wish to bring it forward again before the profession, I will give a sketch of a few cases which have been under my care within the last two years.

CASE I.—An infant of eleven months old, having four teeth, and weaned at eight months, was attacked with English cholera more than

* This irritation may be shown by severe excoriations existing about the anus, of which I have seen several examples.

a week before I saw it. It had a very delicate look; and though the stomach was better than at first, it still, at times, threw off the milk, no matter how prepared, and the diarrhoea was very persistent. The stools were not greenish, nor did they become so. In this state it was ordered three doses of hippo—two grains in each dose, and one of these given each night. Though the first dose bettered the child, still it got the three, by which time it was quite well of both the vomiting and diarrhoea, and being without appetite and otherwise delicate, it then got some pepsine wine.

CASE II.—The child of a doctor got an attack of English cholera, the diarrhoea of which had lasted several days before I saw it. It was a model of a delicate child. Two doses of hippo, given just as in the last case, cured this one.

CASE III.—Wallace, a boy of three years of age, had severe diarrhoea, which had gone on a considerable time. The stools were a very light yellow, and there seemed little or no pain. He was pale, and his appetite impaired. Three doses of hippo cured this case; and it is worthy of note that none of them sickened him. Indeed, generally speaking, the children are not sickened by these doses; but there is a great variety on this point. I observed, however, that when the medicine does sicken, the results are more beneficial.

CASE IV.—Lawless, aged one and a half years, got bad diarrhoea after measles. This child was very much reduced. Three doses here of the hippo cured the diarrhoea, but did not seem to affect the wheezing from which this child suffered.

CASE V.—Reilly, a boy aged fifteen months, had bad diarrhoea, which had been going on an entire month. The attack had begun like cholera. When I saw him he was very thin and very pale, and his flesh flabby to a degree. After getting four doses he was cured, as far as the diarrhoea went. He was not sickened by any of these.

CASE VI.—A child of three years of age, very small and puny, was brought to the Whitworth Hospital, Drumcondra, in January, 1878. She had had bad diarrhoea for three months, and was, as may be guessed, very much spent and pale. She had from eight to ten discharges in the twenty-four hours, and was more frequently disturbed in the night than the day. She had been prescribed for by others. I could not ascertain anything particular about the character of the discharges. This child got six doses of hippo in succession, and with very good results. None of the doses sickened her, and they were then repeated, and at the end of twelve days she might be pronounced well, though weak.

These cases could be added to, but such is quite unnecessary, for more would not, in any way, strengthen the point I have brought forward. That the hippo is very often effective I cannot doubt, but it occasionally fails, and has done so in my own hands. I may mention that in a case

I saw not long since with Dr. Wyse it succeeded at once. I have already stated that it usually does not produce vomiting in the doses given; but on more than one occasion, when it did do so, a quantity of coagulated milk was thrown up, and certainly with benefit. I was not prepared for this. Probably most I address have seen milk thrown up in the shape of a thin rope. Whether in this state it can be digested would seem to be doubtful.

Before I had experience of the treatment of diarrhoea by hippo I was in the habit of using aperients much more than I have lately done, and amongst these castor oil was the chief—not given in the common way, as a teaspoonful or so, but made in emulsion, and given in about ten-drop doses. To this, when there was much pain, I was in the habit of adding bismuth, in doses suited to the age of the child. This combination I have found very useful. Nor, when the case calls for it, is there anything to prevent the hippo treatment being joined to the aperient. In the cases given this evening, however, I wish it to be clearly understood that hippo alone was used.

Of tonics in the treatment of diarrhoea I have little to say. I have not ventured on arsenic, though it has been recommended; but I have seen benefit from the use of oxide of zinc, in grain doses, and also from the pernitrate of iron. Lastly, a change of air has succeeded where all other means had failed.

In astringents, as such, I have very little faith. I am sure they often do harm. At first their action seems beneficial, but this is very often followed by relapses, which occur again and again, and so valuable time is lost. Between the two lines of treatment—aperients and astringents—I would, for my part, infinitely prefer the former.

In concluding these *disjecta membra* I would wish to throw into a series of propositions the few points I have thought it worth while bringing under your notice this evening:—

1. That the morbid changes found in fatal cases of diarrhoea in infants and children do not account for the great obstinacy the disease frequently exhibits.

2. That we are therefore driven to the conclusion that the cause must be constitutional.

3. That this constitutional state is frequently due to the strumous diathesis, and more rarely to the state known as rickets.

4. That we must never overlook the epidemic tendency when it exists, which is a something that acts totally independent of the food, water, or milk, used.

5. That the method of feeding infants, both naturally and artificially at the same time, is to be avoided when possible.

6. That calomel is a medicine which has fallen too much into disuse, and more particularly in what may be called the acute diarrhoea of infants,

marked by greenish discharges, and in which the drug has been found to be of signal benefit.

7. That hippo, used a century since for the same purpose, will be found a very effective remedy in the diarrhœa of the young.

8. That astringents are of less value than aperients in the treatment of this diarrhœa.

9. That tonics, such as the pernitrate of iron, or oxide of zinc, are often of essential service, and above all, a complete change of air.

DR. M'CLINTOCK agreed with Dr. Kennedy with reference to the use of astringents. He himself had changed his practice in this respect. In the case of diarrhœa there often was an accumulation of fœcal matter that should, in the first instance be removed. There was a remedy that he had used with good effect of late years, when obliged to employ an astringent, and that remedy was oxide of zinc, in half-grain doses, in a mucilaginous mixture, every three or four hours. The most important point in these cases was to regulate the diet, and to give the strictest directions to those having charge of the child. He thought that hippo was a most valuable remedy, and not as often employed as it should be. As to the use of farinaceous foods before children are teething, it is to be shunned and avoided.

The PRESIDENT considered all starch foods injurious to the child before the teeth came. It was then a carnivore, and should get nothing but milk. The use of other foods was a fruitful source of disease.

DR. KENNEDY, in replying, said that Dr. Faussett, of Clontarf, had used shell-cocoa boiled and given with half water half milk, with good effect, the child fattening on it very well. Hippo does not act as an emetic in the majority of cases, nor is it exactly an astringent; it is sometimes found to act as an aperient.

METHOD FOR THE DISCOVERY OF SPERMATOOZOA IN URINE.

M. ROUVIER (*Gaz. Hebdomadaire ; Le Praticien*, March 3, 1879) allows the urine to stand for twelve hours, adding benzine in summer to prevent putrefaction; he then decants it and collects the flocculent precipitate. This precipitate is put into a test-tube, ether is added, and the mixture is well shaken. In a few minutes the ether collects on the surface of the liquid as a gelatinous layer. The ether is removed by means of a pipette, and is placed in a conical glass containing a small quantity of distilled water. All the spermatozoa in the urine thus tested are found in a very small compass, and each microscopic preparation will contain at least five or six in the field.—*The Practitioner*, April, 1879.

THE DOCTOR
SOCIETY FOR
MEDICAL
OBSERVATION

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF
DUBLIN.

President—WILLIAM MOORE, M.D.

Secretary—E. H. BENNETT, M.D.

Bilobed Vesical Calculus.—MR. WHEELER said: This is a large calculus of unusual shape, which was removed after death from the bladder of a patient in the City of Dublin Hospital. He was admitted on the 9th of April, 1878, under my care, and stated that for fifteen years before his admission he had been subject to a constant dribbling of urine; had been unable to pass water voluntarily, and had not had the power of retaining it in his bladder, in consequence of which he had been obliged to allow it to pass down his legs in the most convenient way to himself. When admitted into the hospital he was suffering from albuminuria, with pus and blood in the urine. There was no difficulty in finding out that there was a calculus in the bladder; but it was impossible to get an instrument to pass into the bladder, from the way in which the stone lay. The sound hit against the mass, and would not go over it. It was found, on a *post mortem* examination, that the bladder, for two inches or more behind its neck, was contracted tightly round the stone. It was very difficult to get a history of the case, but he stated that he had been dribbling his urine for fifteen years, and it would appear that the formation of the stone took about nine years. I endeavoured to build up the patient's strength, in order to get him into a condition for an operation, but was unable to do so. The albuminuria lessened, but he died comatose from suppression of urine. On the *post mortem* examination the bladder was found to be sacculated, and also ulcerated in parts. The two ureters were so enormously enlarged that I could put my two fingers into them. The pyramids of the kidneys were tightly compressed, and the cortical substance diminished. There was pus in the right kidney, and a large abscess in the cortical and medullary portion of the right kidney. I have not made any section of the stone, as I thought it desirable not to spoil its appearance. I have seen stones of larger size, and which weighed more than this, which, however, weighs three ounces and two drachms. Its larger circumference is $8\frac{1}{4}$ inches, and its lesser $5\frac{1}{2}$ inches. It appears to me to be chiefly phosphatic in composition.—
December 14, 1878.

The following is the report on the composition of Mr. Wheeler's specimen of calculus by Dr. E. H. Bennett, to whom it was referred:—
“The weight of the lobes of the stone which have been separated shows

that they are nearly exactly equal. A section through the centre of each demonstrates that the original nucleus lies in the vesical lobe projecting towards the constriction, while the urethral lobe has been formed by continuous deposits from the extremity of the nucleus which projected towards the urethra. The entire stone is formed of mixed phosphates, chiefly ammonio-magnesian, deposited in crystals, separated into layers by bands of mixed lithates and oxalates. The lithates and oxalates are most abundant in the central parts, the phosphates in the external.—*January 18, 1879.*

Complete Occlusion of the Vena Cava Inferior ; Malignant Disease of the Liver.—Dr. T. E. LITTLE said: Pathological obliteration of the inferior vena cava is an event of the extremest and most exceptional rarity, even as contrasted with similar obliterations of other veins of prime size. The well-known, remarkable, and classical case recorded by Reid (*"Phys. Anat. & Path. Researches,"* p. 509, 1848), in which the inferior vena cava opened into the vena azygos, being regarded as a congenital deformity, can hardly be considered as coming under this category.

The drawing, cast, and morbid specimens, which I beg, on the present occasion, to lay before the members of the Society, illustrate the most noticeable particulars of a case in which this result has occurred, as the consequence of malignant disease (so-called "Farre's Tubercle") of the liver.

CASE.—J. H., a labourer in a timber yard, aged twenty-six, of previous excellent health, and temperate habits, was admitted to Sir Patrick Dun's Hospital, March 4th, 1878. He had felt quite well, and was at work, until a couple of weeks before admission, when his illness commenced with complaint of pain, soreness, and swelling of the abdomen—the right hypochondrium being more especially tender; loss of appetite and condition, occasional sick stomach, and slight shortness of breath. At the time of admission these symptoms were becoming daily more urgent and distressing. The most remarkable feature of the case then, however, was the appearance presented by the surface of the trunk. Over the surface of both chest and abdomen numerous large superficial varicose veins stood out prominently apparent, some almost of the size of the little finger, and forming a more or less close venous net-work over the surface of these regions. They were largest and most copious on the front, and along the lateral axillary line; less marked, though still present to some extent, on the back. The superficial veins of the groins, penis, and scrotum were, moreover, much dilated. In all these veins, which tended to assume more or less generally a vertical direction, the course of the blood could be made out to be from below upwards. The head and neck and upper extremities were entirely free from varicose veins, and although slight varices, and the traces of an old varicose ulcer on the

right shin, existed on the legs, these were plainly unconnected with, and independent of, the system of varices just mentioned. There was *not* to be observed in this case that peculiar condition of dilated superficial veins around the umbilicus to which writers have fancifully applied the term "*Caput Medusæ*," the region of the umbilicus being, in fact, exceptionally free from varices, as were also, to a greater or lesser extent, the mammary and hypochondriac regions. There was no detectable ascites. There existed distinct evidences of enlargement of the liver, as indicated by slight protrusion of the lower region of the right side of the thorax and right hypochondrium—the right side of the body at the zone, midway between the ensiform cartilage and umbilicus, measured one inch ~~more~~ than the left; dulness, on percussion, of the lower region of the thorax on the right side as high as the fourth rib; and descent of the anterior edge of the liver abnormally below the margin of the ribs. There was a very slight icteric tinge of the surface, and the urine contained bile pigment.

In the after-progress of the case the symptoms above detailed became gradually aggravated, and in addition other symptoms appeared. In a note taken March 30th, I find that there then existed a slight but decided amount of ascites, which, along with the upward pressure of the enlarged liver, caused the patient considerable distress of breathing, especially on exertion and at night. The hepatic enlargement had evidently increased a good deal; and at a certain point between the seventh and eighth ribs there was to be observed a curiously localised bulging of the intercostal space, which afforded a spurious feeling of fluctuation, and which was all the more remarkable as a feature of the case in consequence of the increasing emaciation of the patient having rendered the intercostal depressions otherwise in general particularly well marked. The protrusion alluded to presented no pulsation. Another peculiar physical sign observed at this period was the existence of a remarkable, dull, tympanitic resonance on percussion of the right infra-clavicular region, such as—and, I presume, having the same explanation as—the similar curious phenomenon which has been observed in the same region in cases of rapid pleural affusion ("*Skodic Resonance*;" see Trousseau, Tome I., p. 608, ed. 2). This, the right, side now measured an inch and a quarter more than the other.

Subsequently to this the ascites increased; jaundice became more decided; and the shortness of breath became a most distressing symptom. On April 11th he was attacked with retention of urine, and required for some days the passage of a catheter. The urine so drawn off was of a very high colour, deeply stained with bile, and deposited, on cooling, a copious sediment of brilliantly pinkish-coloured lithates. On April 14th the distension of the abdomen from ascites, which appeared to have increased with exaggerated rapidity during the last few days, had produced such

urgent distress of breathing, that on the evening of that day I was obliged to tap the abdomen. The fluid drawn off measured 7½ pints; it was of a very high colour—the high colour in part due to the presence of bile, but also in a large degree owing to the admixture of blood.

The tapping was followed by considerable temporary relief of the more distressing symptoms. A new, but easily intelligible, symptom now showed itself, and gave the patient much distress—viz., a severe stitch of the lower part of the right side. This was accompanied by a loud friction sound heard during the movements of respiration over the lower regions of the right side of the thorax.

Becoming gradually weaker and more emaciated after this, the patient died on May 13th, seventy-two days after admission to hospital, and within three months, at most, of the first complaint of any symptom.

Autopsy (five hours after death).—On making a *post mortem* examination, the superficial varicose veins were still apparent and prominent, and full of blood, as during life. The protrusion at the seventh intercostal space had, however, subsided. I opened the abdomen first, in order to get a correct idea of the exact situation of the diaphragm. On the right side its vault reached as high as the level of the upper border of the fourth rib. The abdominal cavity contained a considerable quantity of ascitic fluid. This fluid had the same qualities and appearances as that drawn off during life—it was high coloured and opaque, contained a large amount of bile, and was deeply blood tinged. There was no evidence of general peritonitis, either recent or remote.

On investigation of the condition of the liver in detail:—As to its *position*, the upper surface reached upwards to the height indicated by the elevation of the right vault of the diaphragm; its anterior edge was depressed to the extent of between three and four inches below the margin of the ribs. On removing it, I found that it had not contracted any adhesions to other neighbouring abdominal viscera, but was, to a limited extent, adherent by old bands of adhesion to the diaphragm above and behind, at and behind the situation of attachment of the lateral hepatic ligaments. I removed, therefore, the attached piece of diaphragm with the organ. In *size*, the whole liver was considerably increased (see cast); this increase principally affected the right and Spigelian lobes, and showed itself chiefly in a great augmentation of thickness. In *shape*, the surface was extremely uneven and nodulated, spherical masses of different sizes projecting from it in all parts. The whole organ, as also were these nodules, was deeply stained with bile. One especially large tubercle (so-called) occupied and projected from the upper surface of the right lobe. It was fully as large as an ordinary fist, and on its anterior surface presented a pair of well-marked and persistent sulci—thus plainly indicating it as that which had produced the localised protrusion at the seventh intercostal space observed during life. Another very large tubercle occupied

the Spigelian lobe. For the rest, on making sections in various directions of the liver, its whole substance, deeply as well as superficially, was found to be studded with spherical masses varying in size from that of a pea to that above indicated. These masses varied much in character, when recent, in different places—some were pale-coloured, hard and tough, others soft and pultaceous, many were dark from blood discoloration, and in some actual blood extravasation had occurred—all were deeply bile pigmented. The extent to which these tumours occupied the substance of the liver was such that they certainly formed considerably more than one-half of the entire bulk of the diseased organ. The hepatic tissue left remaining presented perfectly normal appearances, with the exception of the deep yellow colour from bile. The *peritoneum* covering the liver was thickened, and whitish in a few patches; and over the upper surface of the right lobe it was for the extent of a couple of square inches rough, presenting that appearance of early inflammation which has been compared to that of the surface of a calf's tongue. Otherwise, it was in all parts quite naturally smooth and polished and translucent. There was a complete absence of any appearance of that depression of the centres of the superficial tumours, or so-called "umbilication," which has been so often noticed in cases of "Farre's tubercle." On examining the hepatic *fissures*, there is nothing abnormal to be observed in the longitudinal fissure; likewise, the transverse fissure and the structures in it are normal, with, perhaps, a slight degree of thickening of the leaves of the lesser omentum, as it passes to its hepatic attachment. The *vena portæ* is perfectly normal, of rather large size, entirely pervious, and it contained no thrombus. The *gall-bladder* was of rather small size, its peritoneal covering and attachments slightly thickened, contained some healthy bile, and no gall-stones. The *caval fissure* was much narrowed in the centre. It was occupied by a considerable mass of thickened organised lymph; and on a more careful dissection of the vena cava in this situation, I found that the vessel had here become obliterated for an inch of its course. This obliteration has taken place above the openings of the hepatic veins, and just before the vessel pierces the diaphragm; above the highest of the hepatic veins it terminates in a broad *cul-de-sac*, ending in a fibrous cord of about the size of a goose-quill, becoming above widely pervious again, and of apparently normal, or nearly normal, size as it enters the heart. The openings of the hepatic veins are of unusually large size. It will be observed that at each side of this fissure, where the occlusion of the vena cava has taken place, we meet with a couple of the largest of the tubercles on the liver's surface—one of these, in the Spigelian lobe, almost completely overlapping it. The occlusion of the vein is, for the extent above indicated, absolutely complete, and evidently of some standing. In the narrow and tough cord, to which the vein is here reduced, there is no trace whatever of a blood-channel. As connected with this

remarkable and total obstruction of the circulation through the vena cava inferior, I looked with much interest to the condition of the collateral venous circulation of the lower parts of the body. We have already seen that the abrogated function of the vena cava was to some considerable extent discharged by the superficial veins of the trunk. These may, I think, be classified into two principal systems of anastomoses—viz., (1) the epigastric veins from below which anastomose with the internal mammary, and still more efficiently the anterior intercostal veins above; and (2) the iliac circumflex veins below, with the long thoracic veins from the axillary region above. But the principal compensatory circulation was that carried on by the vena azygos. This vein I found to be enormously dilated, being fully as large as an ordinary inferior vena cava, and of the width of a couple of fingers. It was enlarged both in the abdomen and thorax—its intercostal tributaries, too, being of very large size. As to the other organs of the body, they were, in a word, quite normal and healthy. It was plainly a case of disease localised strictly to the liver. The only region outside this organ which the disease had invaded is the falciform ligament, between the folds of which a few small masses of disease, similar to that in the liver itself, existed. The spleen, stomach, intestines, omentum, pancreas, kidneys, and bladder were quite healthy. The abdominal lymphatic glands presented no appearance of disease. The lungs were healthy, and unusually free from pleuritic adhesions. The heart was of rather small size, and there was apparently nothing abnormal in the size or aspect of the opening of the inferior vena cava. The endocardium was deeply stained with bile.

Remarks.—The literature of pathology contains scarcely any cases of pathological obliteration of the vena cava inferior. I have only been able to discover the record of one case of such complete occlusion as in the preceding case—viz., one reported by Gély (*Gaz. Méd. de Paris*, 1840, No. 45), in which the phenomena, both clinical and pathological, presented a remarkably curious similarity to those of my case, and in which, in like manner, the obstruction occurred as the result of “Farre’s tubercle” of the liver. Some few cases of pressure upon, and partial obstruction of, the vein are to be found—e.g., one by Dr. Habershon (*Guy’s Hosp. Rep.*, Vol. VI., Ser. 3, p. 182), where pressure of a hydatid tumour caused “anasarca and varices of the lower extremities;” and a case of cancerous disease by Frerichs (“*Dis. of Liver*,” Vol. II., pp. 287 and 348), where “the vena cava was much constricted in its passage through the liver.”

A curious and remarkable pathological observation made by Hasse (*Path. Anat.*, Syd. Soc., p. 33), in reference to the greater relative proneness of the pressure of malignant tumours to produce comparatively rapid obliteration of veins, is borne out in the preceding case, the progress of which was exceptionally rapid. He says:—“To effect this coagulation,

however, particular circumstances must co-operate with the pressure. For we may observe, for example, the gravid uterus so to compress the iliac veins as to cause oedematous swelling, varices, and stagnation of the blood without coagulation taking place, whilst it is readily occasioned by the pressure of a uterus enlarged by cancerous degeneration."

It appears, further, that there are two different methods in which tumours may produce obstruction and occlusion of a vein in their neighbourhood—viz., (1) by the actual growth of the tumour into the calibre of the vessel; and (2) by the effect of pressure, causing either adhesive inflammation of its walls or thrombosis of its contents. To the latter of these causes we must attribute the occlusion in the present case. There is no appearance of the growth of any of the diseased masses into the cavity of the vein; and on an examination of sections of the liver, I find no place in which any outgrowth has occurred into any of the vessels whether of the portal or hepatic system of veins. The occlusion has taken place, as I have said, at a point where two of the largest nodules in the liver would, one on each side, strongly compress the vessel.

The absence of the so-termed "umbilication" of the tubercles in this case may, I think, be explained in the following way:—The phenomenon referred to I take to be the result of atrophy of the central parts of the tumours, and the great rapidity of the course of the disease in the present instance was such as to lead probably, I think, to the death of the patient before the time for the development of the atrophic process.—*December 21, 1878.*

Dislocation of Costo-sternal Articulation, and Fractures of the Ribs.—**DR. BENNETT** said: This cast represents the deformity produced by a dislocation of one of the costal cartilages forwards on the sternum in a case of injury of the thorax some little time ago under my care in Sir P. Dun's Hospital. I have here also the dissected specimen. Catherine Flynn, a woman aged about fifty-six years, was admitted on the 27th of November, 1878. A runaway horse with a cart (without springs) had run over her, and she had sustained a fracture of the left clavicle and a lacerated wound on the right frontal protuberance, together with an abrasion of the right forearm, and also a superficial wound over the right tibia. The second, third, fourth, and fifth ribs were fractured on the left side, the third being broken in two places, and dislocated at the sternum. On the right side the second, third, fourth, fifth, sixth, seventh, eighth, and ninth ribs were broken. The fractures were at distances of from one to three inches from the costal cartilages, and many of the bones were broken in two places. She died on the 5th of December. On a *post mortem* examination the left pleura was found to be full of blood, and the heart and pericardium to be slightly bruised. The woman died from the suffocation of pleuritis and pneumonia

consequent upon the injuries she had received. Extensive fractures of the ribs are unfortunately of common enough occurrence. The interest of this specimen consists in the fact that it presents, first of all, an extremely rare dislocation—that of the costal cartilage forward on the sternum; and, secondly, abundant specimens of fractures of the ribs from indirect violence. Sir Astley Cooper, in the last editions of his work on Fractures, by B. Cooper, page 521, appears to deny the existence of this luxation. He says:—"The termination of the cartilage which is at the sternum sometimes projects from a similar cause (constitutional weakness), giving rise to the same false impression upon the minds of the parents, that the circumstance must have arisen from accident, and not from disease." Malgaigne, who has examined into these injuries more closely, has collected only four examples of the dislocation in question. Hamilton quotes the two previous authorities, without adding anything to what they state. Gross describes the case of a male, aged sixty, who fell from a scaffold ten feet high upon stone steps, and sustained a dislocation of the last three ribs from the sternum. He says:—"I have myself seen several cases of the kind, one of which I attended." But he does not give any clear idea of the injury, or indeed to which of three injuries—dislocation of the ribs from their cartilage, of the latter from each other, and from the sternum—his remarks apply. The dislocation is of very little practical interest, except for its rarity and the mode of its production. The present specimen has been somewhat hardened in spirits. When I observed the deformity, although the woman's condition was very low and unpromising, I at once took the cast of her chest. On removing the cast I was able to reduce the dislocation with the greatest facility. Steady pressure of two fingers made for a moment on the extremity of the tumour at once reduced the dislocation, and it exhibited no tendency whatever to recur. I could not get the cartilage and the sternum exactly to a level, but the tumour shown in the cast was entirely removed. The fibrous envelope of the cartilage, with the ligaments attached to it, are stripped off. One of the synovial joints seems to have been imperfectly formed; the upper one is complete, and has suffered a true dislocation; and the fibrous structure obliterating, in part, the lower articulation has been torn through. The perichondrium, with the ligaments attached to it, are stripped clean off. The remaining points of interest are the fractures of the ribs themselves, which are sufficiently numerous. In the instance of the third rib on the left side, where the dislocation of the cartilage occurred, the bone is broken in two places near the sternal end, the lines of fracture being oblique from above downwards and inwards, and parallel to each other, an inch and a half asunder. The fracture in the rib above is further out still towards the axilla, but far forward in the body of the bone. On the opposite side two of the fractures deserve particular attention. All

are placed forward much nearer to the sternum than to the angle of the rib—that is, the whole group of fractures is placed towards the anterior aspect of the chest. This demonstrates anew the fact pointed out by Malgaigne, that fractures produced by indirect violence commonly occur in the anterior segment of the body, but, at all events, in front of the angle. In the vast majority of instances they occur nearer to the sternum than to the angle where Petit and those who follow him would place them. It is interesting to see the character of one of these specimens in its section. Although we cannot, strictly speaking, call this anything like an impacted fracture, still the tendency to impaction exists. At the third rib we have two fractures. The chest wall has been bowed in, and at the posterior fracture the tendency is seen, though it is not so well marked as in other specimens I have recorded. These specimens fully support the view I put forward a short time ago with reference to such specimens—namely, that we are to regard the appearance of overlapping as evidence of the fractures having been produced by indirect violence—by a force acting lengthwise upon the rib, and tending to crumple it up. The lowest fracture on the right side is an instance of incomplete fracture, the outer table of the bone only being broken. To sum up, the principal points of interest in this specimen are, first, the rarity of this dislocation; secondly, its coincidence with these fractures; and, thirdly, this perfect specimen low down of an incomplete fracture of the rib, the bone being broken on one aspect, and perfectly intact on the opposite aspect—a form which Malgaigne has thought worthy of being both noticed and figured in his book. I should have mentioned with reference to this dislocation that there is no record of any dissection in the recorded cases.—*January 18, 1879.*

Strangulated Femoral Hernia.—DR. E. W. COLLINS said: The pathological specimen I exhibit was obtained under the following circumstances:—During the recent severe weather my colleague, Dr. MacSwiney, was summoned to St. Joseph's Asylum for Aged Females, to see an old woman who was suffering from very severe bronchitis. On visiting her he also learned that for three days she had had severe vomiting, and no movement of her bowels. Further inquiry led him to the conclusion that she had a strangulated hernia. He thereupon ordered her to be transmitted to Jervis-street Hospital, and placed under my care. On examining her shortly after her admission to hospital, I found her in a very prostrate condition. Her pulse was very feeble and rapid; her skin cold; her breathing laboured and rapid, owing to severe capillary bronchitis. On careful manipulation I discovered a small tumour of the size of a hazel-nut in the left femoral fossa. It was slightly tender, elastic, but partially reducible on pressure, and received no impulse on coughing. The abdomen was greatly distended with flatus. While I

was handling the tumour very characteristic faecal vomiting occurred, which, together with the rapidity of her pulse and the physical signs already enumerated, left no doubt on my mind that I had to deal with a small and acutely strangulated femoral hernia. I afterwards learned that the woman had been a nurse, and during the last thirty years had suffered from the presence of a small hernial tumour in the left groin. Twice previously it had become strangulated, but had been reduced without much difficulty. Finding the taxis ineffectual to reduce the tumour, I ordered castor-oil and turpentine enemata to be carefully administered with the long tube, while stimulants were exhibited both externally and internally to bring about reaction. The first enema brought away some faecal matter. At a later hour my colleagues met me in consultation. Meanwhile the respiratory distress, consequent on the bronchitis, had become much augmented. She was semi-cyanotic, pulseless, and, in fact, moribund. We, therefore, came to the conclusion that operative interference, under the circumstances, would be unjustifiable and useless. She died shortly afterwards. The autopsy revealed an old femoral epiplocele, the size of a Spanish nut, perfectly adherent to its sac. A small portion of the jejunum, recently strangulated, was adherent to it on its posterior aspect, lying between the bone and the omental protrusion. The specimen still exhibits the remains of these adhesions. The amount of intestine protruded and strangulated comprises only a portion of the lumen of the tube, and, from its pouched appearance, must have long occupied the femoral ring and canal. The pouch is still clearly seen. No doubt this small portion of the intestine had frequently slipped down into the femoral ring behind the knuckle of omentum permanently fixed there, and had receded or been reduced from time to time. I think it probable that, during some paroxysm of coughing consequent on the severe bronchitis from which she latterly suffered, some more of the intestine than usual was protruded and nipped in the ring; that it then became strangulated and adherent—finally, that the intestine became paralysed, owing to inflammation, so that its obstruction, though at first partial, thus became complete. The case is instructive in this respect as demonstrating how strangulation and obstruction of only a limited portion of the calibre of the bowel may lead to complete obstruction, most probably as the result of its inflammation and consequent paralysis.

The CHAIRMAN.—I think Dr. Collins' observations as to the small amount of strangulation that may produce obstruction are very important. It has been long known that strangulation of the omentum may occur without the gut being included at all, and may exhibit symptoms very similar to those of entire obstruction of the bowel, such as we ordinarily meet with in cases of strangulation of the intestine. The symptoms are hard to account for, except on the supposition suggested by Dr. Collins of paralysis of part of the gut. The only question connected with the

case is whether the pouch given off from the intestine may not have been a congenital diverticulum.

DR. FINNY.—Paralysis of the bowel may be produced by other causes than a portion of it being nipped. According to Abercrombie, under the influence of strong purgatives a portion of the bowel may become paralysed. Dr. Stokes published a case showing the advantages of electricity as applied to cases of this kind, in which that agent proved the means of saving the life of a patient after the most determined and prolonged obstruction of the bowel.—*February 8, 1879.*

ON THE USE OF THYMOL.

DR. SEYFERTH has published the results of his experience with thymol, which he has latterly employed exclusively in all surgical cases, and also in other cases requiring local antiseptis. In several cases of extensive burns it acted very satisfactorily, stopping at once the offensive discharge in cases that had been treated for days with carron oil, relieving the pain entirely, and producing a rapid recovery. The deep sloughs separated more rapidly than usual, and the granulations were not so exuberant as in cases treated by carbolic acid; the resulting cicatrices also were not so deep and firm. A violent stomatitis, due to the inhalation of caustic vapour, which had been treated for two days without relief by potassium chlorate and solutions of boracic acid, was quickly cured by a solution of thymol, of the strength of 1 : 3000. The same solution proved very effective in seven cases of diphtheria, three of which were very severe, and accompanied by great foetor of breath. Here it was used by injection into the nose, as well as into the mouth. Purulent coryza and suppurative aural catarrh do well under the thymol treatment; solutions of 1 : 4000 are well borne, and do not irritate either the nose or the auditory canal. Two cases of ophthalmia neonatorum were cured by the thymol solution, with which the eyes were washed out every hour; between the washings the eyes were kept constantly covered, at first with ice-compresses, and later with compresses of cotton soaked in the thymol solution. In some puerperal cases with offensive lochia, the vagina was washed out repeatedly—at first with solutions of 1 : 1000, and later with solutions 1 : 2000 and 1 : 4000; the results were rapid deodorisation and a favourable action in the general condition of the patients. In two cases of old vaginitis, with profuse, stinking discharge, a cure was obtained in fourteen days by irrigations with a solution of 1 : 4000, repeated three times a day, and followed by the introduction into the vagina of plugs of cotton dipped in thymolised glycerine. Dr. Seyferth keeps the following solution constantly on hand, and dilutes it to the desired degree whenever it is needed:—Thymol, 1·0; spir. vini, 10·0; glycerin., 20·0; aquæ destil., 70·0 M.—*Allg. med. cent. Zeit.*, No. 62, 1878, and *N. Y. Med. Record.*

CLINICAL RECORDS.

Report of a Case of Injury to the Nervous Centres, caused by a fall from a vehicle in motion. Communicated by S. M. MACSWINEY, M.D., F.K.Q.C.P.

IN the number of *The Dublin Journal* for December, 1878, there appeared the history of a case of "Railway Spine," which terminated fatally, and which was communicated by me to the Medical Society. Some weeks after the publication of this case, I received a letter from a medical gentleman, of which the following is a short extract:—"A few days ago I saw a short notice of a paper which you had read before the King and Queen's College of Physicians, on Nervous Lesion from Shock. Thinking you would like to hear of some more of these cases than you have yourself witnessed, I take the liberty of sending you a report of one which is, unhappily, but too well known to me—I myself being the sufferer." Accompanying the private letter was the following case, which I give without note or comment—feeling satisfied that everyone interested in the history and progress of nervous lesions will find, in the minute and graphic delineation of the features of the case, supplied by the writer, ample materials for forming an opinion upon the essential nature of the affection from which he has suffered so much.

CASE.—Jan. 1, 1878, on returning from visiting a patient some miles off, the night being very dark, my horse took fright, bolted, and flung me out of a sulky, or light gig, to the dry bottom of a ditch, the ground being rough and frozen very hard. I fell flat on my back. I felt little or no pain in my back, but two days after I became weak, and on coughing or sneezing felt a most excruciating pain, rather deep-seated, behind the fifth and sixth interspace on the left of the sternum. This continued, more or less, for, I think, about a fortnight, when it nearly all went away. Occasionally I have felt it since, particularly when lying on the right side.

During January I grew weaker; legs seemed to fail; palpitation came on when I exerted myself. Wrote to a medical friend on the subject. He thought the trouble was a feeble heart, and that I had been weakening myself by taking anthelmintics to remove a *tænia* with which I had been troubled. He prescribed iron and digitalis, I believe. The digitalis I found I could not bear, even in small doses. It affected the nervous system fearfully, so that I could scarcely steady my hand to write. I continued to get weaker for a few weeks, and once had to lie down when visiting a patient, I felt so much prostrated. About March 23rd, I was obliged to give up and take to my bed. My sensations were—a feeling

as if the ears were plugged; a sense of congestion about the medulla oblongata; a quivering or dizziness frequently occurring, and scarcely describable, apparently in the cerebellum and posterior portion of cerebrum, as if a fit of some kind were impending; once or twice a sudden acute musical sound in the ears, then a buzzing, chiefly in the left ear, which, varied in sound, continues still. This tinnitus sometimes was, and is now, like water flowing at a distance; sometimes like the noise of a boiling kettle; sometimes like frying meat; sometimes like a fife. Appetite tolerably fair; exhaustion tremendous. Had to keep the horizontal position. On sitting up for a little I got faint, sometimes so much so as to be almost sure I was dying. Got scarcely any sleep; when I dozed a little, horrible dreams and sensations like nightmare suddenly awoke me. Pulse weak, tolerably slow and regular. Great venous congestion in hands, arms, and feet, probably from impaired power of circulation. The doctor kindly visited me. He still regarded the heart as the cause of my illness. Though a skilful man he did not appear to suspect the real cause of the suffering, but rallied me; told me to get up out of bed; to eat plenty of red meats, use stimulants—and, indeed, talked to me as if I were a hypochondriac. I asked him did he not think that I was suffering from a lesion of innervation. He assented to this in such a manner as if he did so to please me. I was ordered a syrup of triple phosphates—iron, quinine, and strychnine were the active ingredients. One or two doses was all I durst take. The strychnine played havoc with me, and made my head far worse.

Gradually the attacks of prostration and faintness wore away, but I suffered fearfully from my head. When able to be out of bed a few hours in the day I used to go to the pump and place my head under the stream. Sometimes I would get pails of water, intensely cold, poured on my head to relieve the vertigo and pain.

Matters continued thus till about the end of April, when I suddenly felt a slight pain in the sacrum. This got worse in a day or two, and I had to dry-cup myself. I felt a little better, and having business as a witness at the county assizes in a murder case, I ventured to go to court. Whilst there I suffered a good deal with my back. I should not have ventured out had I not thought I was suffering from lumbago, several attacks of which I had had previous to this illness. On my return I fomented my back with very hot water, and felt much relieved next morning. But in about ten days from the inception of this attack in the loins I was seized with a pain of a very intense kind, on the right side of the spine, near the last of the dorsal vertebræ. I again dry-cupped myself, and had several times to use hypodermic injections to assuage the suffering. The pain passed down the course of the anterior crural nerve through the iliac region to the thigh. Spasmodic action of the femoral muscles followed, and then a paralysed condition of them. When

I attempted to walk the right limb could not support my weight. I ought to have mentioned that when the head symptoms commenced, fibrillary tremors in different parts of the body followed, and these have continued ever since. The muscles of the right thigh became a little stronger, so that I could go up and down stairs, but the pulse got quicker, pain continued in the back, then atrocious pains came into the hips and legs—afterwards into the hands and arms.

In June I consulted two eminent specialists. One of them pronounced the case one of *locomotor ataxia*, and prescribed ergot (which I had been taking) and bromide of sodium. I had previously blistered, cupped, leeches, and used strong purgatives, but did not begin soon enough, in consequence of the feelings of prostration and the dictum of my medical attendant as to the diagnosis and treatment. The other physician, seeing that I could stand with my feet close together, and my eyes closed, and walk straight without losing my sight, denied this, and said he thought the trouble was in the meninges. He prescribed salicylate of soda. I did not take much of this, but persevered with the fluid extract of ergot for a month and more. I had to substitute potass. iod. for the sodii bromid., in consequence of an eruption coming out on my face. When the femoral muscles were paralysed I experienced almost complete anæsthesia on the front of the right leg, from knee to ankle. I blistered the surface with considerable benefit. Afterwards there was hyperæsthesia in right knee. By advice of a physician I used dry-cupping over the spine daily, for about a month. I have been repeatedly blistered, and have had blood drawn from the region of the spine. Last November I went to a watering place where there are sulphur springs. I drank the waters and had baths, but with the effect of making me weaker. The “pack” alone seemed to benefit me; the lumbar pain appeared to be alleviated by it. I took, under medical advice, the steam bath, the Turkish bath, the electro-chemical bath, &c., but had, at last, to protest against them. One of the specialists in neurotic diseases ordered me the induced current—to be used frequently—but electricity did not seem to suit; probably there was too much congestion, and the lesion was too recent. It seemed to make my head worse. In the earlier part of my illness I had perineal pains, and a little irritation of the bladder; these went away after a time almost entirely, but large quantities of urine—for the most part light-coloured and clear—were passed; bowels rather constipated; have had to keep in bed for months. The present symptoms, nearly thirteen months after the accident, are:—Pulse, when I am quiet and in recumbent position, 80—compressible, regular; when I get out of bed, or use exertion, 120; occasional prostration, with fluttering of heart—this almost always occurs when awaking from sleep, perhaps partly from mental anguish and disappointment, on awaking and realising my condition; tendency to

melancholy; appetite fair; pains not so bad as formerly; muscles of thigh stronger; can walk pretty well, though feet feel numb at starting; soreness and occasional pain in knee-joints; abundant pale urine; muscles generally flaccid; fibrillary tremors continue, but not so bad as formerly—the most annoying are around and in the ears; neuralgic pains often shoot into various parts of the face—sometimes to ends of toes and fingers. Formerly there appeared to be a deficiency of synovial fluid, particularly in maxillary and cervical articulations, producing cringing and cracking noises; there appears now to be a better supply. Headache and vertigo not so bad; tinnitus continues; dry mouth and thirst at night. Always confined to bed, (1) because the back and limbs seem easier in the horizontal position; (2) because the glutei muscles being wasted it hurts me greatly to sit, the os coccygis not being kept up from the seat by the cushion of muscle; (3) because I have lost energy to do any work, and I seem to bear solitude and idleness better in bed. I still suffer from want of sleep; the trouble of mind and the want of exercise will account, in some measure, for this. If I lie on my side the foot which is undermost will sometimes become partially numb. If I lie on my back the ring and little fingers become numb from the slight pressure on the ulnar nerves at the elbow. Among the pains which I formerly felt were some of a burning character, and apparently in the integuments; these have nearly quite disappeared. Standing long, or walking much, causes more after-suffering. I am taking no medicine, and eating moderately. I have in former years been, in the autumn, subject to bronchitic asthma, and since this illness came on I seem to be more susceptible of these attacks, whenever I catch cold.

Remarks.—On reviewing my case I am inclined to think that there was concussion, if not contusion, of a portion of the nervous centres, which finally induced inflammatory action. Had I been near to an experienced physician he might have detected this while in the stage of meningitis; and, probably, had it been combated by purgatives, local blood-letting, blistering, calomel and opium, &c., it might never have advanced to the point it did. When the violent pain came on, about the tenth day, hyperæmia, probably, had so far advanced that sanguineous effusion took place; if not, perhaps, the effusion was serous from inflammatory action. Unfortunately, I was in an isolated position and had to do almost every thing for myself—illness, no doubt, obscuring the judgment, and absence of hope causing apathy and carelessness. The value of the services of an experienced brother *medico*, who will *con amore* stand by us when we are ill, is very great.

I may add that I was in tolerably good health when I received the injury, only run down a little by hard work. I have lived a temperate life and am of robust *physique*, or rather was so before the muscular system suffered, and something beyond middle age.

COMMENTARIES ON DISEASES OF THE KIDNEYS.*

PART II.

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IN the treatment of nephritis the double indication is to relieve the kidneys and to promote the action of the other excretory organs. In a case of acute nephritis it is necessary to keep the patient in bed if we wish to render the prospect of a rapid and favourable course more certain. The patient should be at once confined to bed in a room warmed to a temperature of at least 60° F., swathed in flannel, and made to lie between the blankets. By thus maintaining a uniform and constant warmth of the skin, we avoid those fluxions to the kidneys which, as physiological experiments show, are associated with the cooling of the cutaneous surface. Blood may be abstracted from the loins by cups or leeches in proportion to the acuteness and severity of the attack and the age and strength of the patient. After the abstraction of blood, large hot linseed-meal poultices should be applied to the renal regions, and renewed every three hours. In sthenic cases, where the fever runs high, and there is severe headache, venesection may be practised with advantage; it is more likely to be required in such cases as come on from cold, and attack plethoric persons in their usual health, than if the nephritis be a sequel of some severe febrile affection, as scarlatina. The readiness with which the kidney relieves itself by a copious hæmaturia often makes artificial depletion unnecessary, even where it might otherwise be thought desirable. The method of treatment pursued by Bright—writing in the year 1827, and again in 1836—at the commencement of the disease was “general bleeding, freely practised, and quickly repeated.” A study of the cases he reports illustrates the discriminating and cautious manner in which this most eminent physician employed blood-letting. The action of the skin should be promoted by the use of

* The text of the essays of Professor Carl Bartels, of Kiel, and Professor Wilhelm Ebstein, of Goettingen, in Volume XV. of the “Cyclopædia of the Practice of Medicine,” edited by Dr. H. Von Ziemssen, has been followed as closely as possible, with additional notes from all the best authorities on the subject.—A. W. FOOT.

hot-air or hot-water baths. A hot-air bath or a hot-water bath should be administered every evening or every second evening, and followed by envelopment of the body in warm blankets. When no conveniences for a hot-water bath exist, an excellent substitute is found in the "blanket-bath." A large thick blanket is wrung as dry as possible out of boiling water, and wrapped round the body of the patient; the bed clothes are then heaped on. In twenty minutes or half an hour, the hot blanket is removed, and the surface of the body quickly dried with a warm soft towel. The production of active diaphoresis (by means of hot baths) in an acute inflammation of the kidneys, which had caused great diminution of the urinary secretion, is followed at once by a marked increase of the urine flow. The artificially-produced congestion of the cutaneous vessels necessarily helps to bring the stagnant blood in the renal vessels again into circulation, since the natural obstacle to their disgorgement presented by the distension of the general venous system must necessarily have been diminished by the diversion of large quantities of blood to the surface of the body. It is in this way that the induction of a diaphoresis contributes to the re-establishment of the normal condition of the circulation through the inflamed kidneys, and thereby also to the re-establishment of the normal secretion. A really efficient diaphoresis can be accomplished only by heating the skin through some agency which acts on it from without. The method which is most agreeable to the patient, and at the same time also most effective, is to heat the skin by dry hot air, as is done in the so-called Turkish bath. This plan is greatly to be preferred to vapour and steam-closet baths, since thereby any actual overheating of the body is avoided, although the atmosphere of the *sudatorium* is commonly raised to above 50° R. (145° Fahr.) The sweat which pours freely from the whole surface of the body when such a temperature as this is reached rapidly evaporates in the dry hot air, and the heat required to convert the palpable moisture (standing on the body in the form of drops) into vapour is extracted in part from the surface of the body, and in part from the layer of air which lies next against this. In a private house an imperfect substitute for the hot-air chamber may be contrived by seating the patient upon a wooden

and putting one or two spirit lamps on the lamps have been lighted, blankets should in such a manner as to cut off the unding him from that of the room at ft exposed. An inexpensive and very ly obtainable wherewith a very perfect d easily administered.

mployed care should be taken to manage not merely warm baths. According to should be placed daily in a hot bath (at

a temperature of 38° Cent. or 100·5° Fahr.), and the temperature of this should then be gradually raised, if the patient can bear it, up to 42° Cent. (107·8° Fahr.) by the addition of hot water. The patient should remain in it as long a time as he comfortably can (not beyond a full hour); the room being thoroughly warm, he should then be as quickly as possible packed in previously heated blankets, and be kept wrapped up in these for one or two hours on a couch or convenient lounge; he is then, finally, to be rubbed down quickly, and put into a previously warmed bed.

Of medicinal diaphoretics, citrate of potash draughts, given every two hours in effervescence, may be serviceable, or a mixture of the liq. ammon. acet. in two or three drachm doses, with fifteen drops of tincture of hyoscyamus in an ounce of infusum lini. Stimulating diuretics are to be carefully avoided, as they are liable to exasperate the existing congestion and to produce hæmaturia. Of all diuretics water is the best. Two or three pints of pure or of distilled water may be taken daily, or soda water or any other similar drink may be substituted. The free administration of water facilitates the elimination of the urinary solids, hence the importance of increasing, if possible, the aqueous part of the urine. An additional reason for the use of so simple and effective a diuretic as pure water is afforded by the character of the disorder, which consists in an exuberant epithelial growth occurring in the tubes; to prevent dangerous obstruction it is essential that a sufficiency of fluid should wash out the accumulated cells. If the tubes can get rid of their contents, the congestion of the gland will be relieved by secretion, the system cleared of its impurities, the organ gradually restored to its healthy state, and recovery ensue.

Digitalis is a remedy which is often of great value in grown persons or in children when the disease is severe. It adds to the force of the heart, and increases the *vis à fronte* in the Malpighian filters. The best preparation is the infusion. The doses may vary from one to four drachms, according to the age of the patient, repeated twice or thrice in the day, or in severe cases every three or four hours. Digitalis may also be applied externally in the form of cloths steeped in the infusion and laid over the abdomen. Sir Robert Christison recommends a combination of digitalis and bitartrate of potash as superior to either remedy given singly:—"The former was usually given in the dose of one or two grains of the powder, in the form of a pill, three times a day, or in the dose of ten, fifteen, or twenty minims of the tincture, three times daily, in a little distilled water of cinnamon or cassia. The cream of tartar was administered thrice a day in the quantity of a drachm and a half, or two drachms, with about five ounces of water."

Indiscriminate purging with jalap and elaterium is to be avoided, and it will often be noticed to be followed by a diminution of urine and an

increase of the dropsy, the result of the misappropriation of the aqueous fluid which is needed to keep the tubes clear.

The obstinate vomiting which occasionally ushers in acute renal dropsy may be combated with creasote or small doses of chloroform given in iced milk or water, or by two or three drops of hydrocyanic acid in a glass of soda or seltzer water. Severe and intractable vomiting at the commencement of the disease is sometimes due to direct sympathy with the renal irritation, and is probably a reflex symptom provoked by the irritation of the nerves in consequence of the acute swelling of the kidneys. In such cases it is more likely to subside after local depletion of the kidneys than after any medicinal remedies. When it is due to genuine uræmic poisoning it is rarely amenable to treatment.

The diet to be allowed in acute nephritis is a matter of very great importance. The physiological repose of the inflamed organs is to be sought for by cutting off the materials which furnish urea and uric acid. Meat and eggs are to be prohibited, also the use of tea and coffee—substances which are supposed, not without reason, to exert an irritating action on the kidneys. A milk diet, with all its possible variations, would be the ideal bill of fare for a patient in acute nephritis, and to it may be added light farinaceous foods with fruit and vegetables. After the acute stage has passed it is advisable to give iron. If the hæmaturia be of long duration ergot is a most effective remedy. When a favourable issue has been obtained, unusual care is required to guard against relapses, to which the patients continue liable for a considerable period. The slightest exposure is sometimes sufficient to reawaken the pyrexia, and to cause the reappearance of albumen and blood in the urine. A complete suit of flannels is essential, and as a rule the convalescent should not be permitted to leave his room until the albumen has disappeared from the urine. When that comes to pass change of air to a warm, sheltered locality is likely to be highly beneficial, and to hasten the restoration of the impoverished blood.

Hitherto we have not spoken much of Bright's disease, at least under that name. We now approach the chronic diseases of the kidney to more or less of which that term is applied. In proportion as increased pathological knowledge has widened the scope of Bright's brilliant generalisation, in the same proportion has the term become more vague and more difficult of definition. Before Bright's observations it was known that the urine is albuminous in some forms of dropsy, but he demonstrated that this albuminuria is due to disease of the kidneys. Such kidney diseases as produce albuminuria have ever since been called by the general name of Bright's disease; and although the renal changes which agree in producing albuminuria present, as we shall see, several distinct varieties, yet the characters which they possess in common unite them into a practically well-recognised kind. It is not so very surprising,

as at first might appear, that these states of kidney should have long escaped notice, for the kidney itself gives usually no pain, and the urine is clear, while death occurs under widely various circumstances of dropsy, acute inflammation, apoplexy, &c., not one of which points evidently to the kidney as its source. The discovery and proof of the common cause of the vast number of different disorders produced by this disease will ever make Bright's name illustrious. Two main divisions of Bright's diseases, which is a better term than Bright's disease, based on clinical characteristics, may be conveniently made—acute and chronic Bright's disease. The former division embraces a compact and universally recognised group which we have recently been considering, known also as “inflammatory dropsy,” “acute renal dropsy,” or “acute albuminuria.” This typical “acute Bright's disease” occurs, as we have seen, as an independent ailment, associated with albuminuria and dropsy, often caused by cold, and then usually during exposure while the person is hungry or fatigued. We have also considered the similar condition which results from scarlatina, diphtheria, and other severe febrile affections. The latter division, “chronic Bright's disease,” includes the protracted cases which have either passed from the acute form into a chronic state, or, which is far more frequent, have been chronic from the first. The kidneys of persons dying of chronic Bright's disease present three chief types of alteration. In the first the kidney is smooth, white, and enlarged; in the second it is granular, brownish or red, and contracted (the cirrhotic kidney); in the third the kidney is lardaceous or waxy (the so-called amyloid degeneration). The special clinical history pertaining to each of these anatomical types has not yet been made out with sufficient precision to enable them to be invariably recognised during life. Furthermore, these types are not always found simple and unmixed. Hence it comes to pass that a complex anatomical state is produced which is associated with a complex clinical history. The type (No. 1) of chronic Bright's disease whose anatomical character is what Samuel Wilks* first described as “the large white kidney” is spoken of and written on under different descriptive terms. Thus it forms the second stage of the Bright's disease of most writers; the non-desquamative nephritis of Johnson; the chronic parenchymatous nephritis of others. This form of Bright's disease is, in a few instances, developed from an acute inflammation of the kidneys, the result either of scarlatina or of exposure to cold, but in the great majority of the cases it progresses in an insidious manner from the very beginning. Among the causes which seem to operate in inducing this form of chronic Bright's disease are affections which are accompanied by persistent suppuration, malarial poison, or marsh miasm, and the misuse of mercury. But in a considerable number of cases it is impossible, from the past history of

* Guy's Hosp. Rep. 1853.

the patient, to discover any sufficient cause for the development of the malady. There does not appear to be any good reason for believing that alcoholic excesses, to which the disease is by many attributed, can be fairly charged with being the cause of it. A fact about it which is better known than its etiology is that young persons are decidedly more frequently attacked than those who are advanced in years. The average age of 106 cases of smooth large kidney examined by Dickinson was 28·2 years; whereas in 250 cases of granular kidney the average age was 50·2 years. The disease is very rarely ushered in by an attack of acute nephritis; when it is so the concurrent symptoms of acute inflammation of the kidneys are strikingly obvious. When it supervenes in its usual insidious manner, its commencement will only be disclosed by systematic examinations of the urine, which it should be a standing rule to institute in every suspicious case of disease. Nothing betrays the insidious affection of the kidneys in its earliest stage; there is no pain or discomfort of any sort in micturition—the only signs being a diminution in the quantity of urine passed daily, and the presence of albumen. When the urine is examined a diminution in quantity and a large percentage of albumen may be observed for a long time prior to more obvious symptoms, and the excretion will not revert to its normal conditions as long as the disease progresses. In those cases in which no examination of the urine is instituted, and in those in which the disease attacks, without evident cause, persons who were previously in sound health, dropsy is, almost without exception, the first symptom that betrays the malady. When these patients, frightened by the commencing dropsy, seek medical advice, they are generally found to be noticeably pale and anæmic, although they had previously considered themselves perfectly well. They usually state that they have noticed for some time past a diminution of bodily strength, which, however, has not been sufficient to prevent them from pursuing their occupations. As a rule they have experienced no other disturbances of health which would point to any decided organic disease. In a few rare cases they complain of dull pressing pains in the renal region which, masked under the customary cloak of *rheumatism*, fail to excite the suspicions of the patient or of his medical adviser. Once begun, the dropsy is wont to increase rapidly, and usually attains a high grade in spite of all therapeutic measures. Its distribution is the same as that of all the other forms of renal dropsy, the subcutaneous areolar tissue being its chief seat. Beginning either in the feet or in the face, it extends, as a rule, over the entire body, and usually holds its ground obstinately in the parts it has once involved. The external genitals, in particular, are constantly swollen, and often remain swollen for months at a time, so that the foreskin is curled up in front of the penis like a post-horn, and the scrotum presents the appearance of a bladder filled with water, which may be larger than a child's head,

and can then no longer be accommodated between the œdematous thighs. Very generally the abdominal walls are engaged to an extreme degree in the general swelling even before any perceptible quantity of fluid is collected in the peritoneal cavity. The serous cavities do not long remain free from dropsical collections. Large quantities of fluid are found in the pleuræ, the pericardium, and the abdominal cavity—more frequently in the chronic than in the acute form of nephritis, and it is not at all rare for death to be caused by them, when the fluid is not evacuated sufficiently early. The anasarca may attain to such a grade that rupture of the epidermal layer occurs in consequence of the enormous tension of the skin, and the dropsical fluid may trickle out from the small cracks in such quantities as to wet the entire bed of the patient through and through, and collect in pools upon the floor, after filtering through the mattresses. The epidermis over an extensive surface is macerated in this fluid, and thrown off, leaving the corium bare and exposed; this usually happens on the lower part of the thighs, and in the scrotum. In favourable cases a general diminution of the dropsy follows this enormous discharge of fluid. The bare and relaxed corium in the affected spots is covered with pale, glassy granulations, over which a new layer of epidermis is developed. In other cases, however, the spots in which the corium has been deprived of its epidermis become the starting-points of superficial or deep gangrene. Another sequel of the loss of the epidermis, less common, however, than superficial gangrene of the skin, is a phlegmonous inflammation of the subcutaneous cellular tissue, which, as a rule, proves fatal. The mucous membranes, especially that lining the intestinal tract, are also involved in the dropsical swelling. Vomiting of watery masses and profuse watery diarrhœa are symptoms which indicate this condition of the gastric and intestinal mucous membranes. The mucous membrane of the respiratory organs seems to be less frequently affected than that of the intestines. Œdema of large portions of the pulmonary tissue occurs not infrequently, and, as a rule, proves fatal. Pulmonary œdema is one of the more frequent causes of death in chronic nephritis. Besides the sense of oppression caused by increasing difficulty of respiration, the long agony is preceded by a tormenting cough, accompanied by profuse, watery, and frothy expectoration, and râles in the bronchi, which can be heard at a distance; by a constantly increasing cyanosis of the lips which contrasts strangely with the ash-pale colour of the swollen face, with prominent eyeballs; by a disappearance of the pulse; and by a gradually ascending coldness of the extremities. As long as the patients remain free from secondary inflammations in other organs, the chronic parenchymatous nephritis runs its course entirely without fever. The disease is of shorter duration than the granular kidney. In fatal cases the ordinary duration of the disease is under six months. Temporary recoveries and relapses are frequent; in

protracted cases the albuminuria may continue long after the dropsical symptoms have passed away; it may remain copious even for more than a year after all other symptoms of disease have ceased. There is a markedly greater tendency to secondary inflammations and to uræmic accidents than in granular kidney, but less to valvular heart disease and hypertrophy of the left ventricle. As long as the dropsy exists it conceals the extreme emaciation which has set in meanwhile. Sometimes it is astonishing how skeleton-like the previously shapeless and swollen limbs become when the dropsy has entirely disappeared—not only the subcutaneous adipose tissue, but also the muscles having been reduced to the merest remnants.

Digestive disturbances set in pretty early in some cases, and a failing or capricious appetite, or slight dyspeptic symptoms may attract notice even before œdema directs attention to the kidneys. The patients, with especial frequency, experience a positive repugnance for animal food. When complete recovery takes place, which is rarely the case, the patients slowly improve in condition, and it is a long time before they recover their former healthy aspect. The convalescence is more frequently incomplete. A portion of the renal tissue has been destroyed, and what is left continues to excrete albuminous urine. The patients then remain somewhat emaciated; the unhealthy colour persists; the skin is constantly dry; they do not completely recover their strength, and sooner or later death is caused by the consequences of this condition.

Post Mortem Appearances.—When the disease proves fatal, as usually happens, at its height, we find both kidneys very considerably enlarged, and, as a rule, much larger than they ever are in acute nephritis. When only one kidney is present, or when the other has become incapable of performing its functions, the diseased kidney, which may entirely fill the hypochondriac space, presents rather the appearance of a tumour; at all events the enlargement is more excessive than it ever is in cases of cyanotic induration (the state of kidney in passive hyperæmia) and simple granular degeneration, and the organ not infrequently attains double or treble its ordinary volume. The capsule is tightly stretched and gapes widely upon section. These large kidneys are always exceedingly anæmic on the surface; their colour is always strikingly pale, almost white with a strong tinge of yellow. When a vertical section is examined we see that the enlargement of the kidney is principally due to an increase of the cortical substance, which is double or even three times its ordinary thickness. Its colour is ivory white or (in cases of fatty transformation) yellowish. The cones appear conspicuously red from contrast with the abnormal whiteness of the cortex. The microscopic changes are essentially confined to the uriniferous tubes. The epithelial lining of the tubes is enormously increased in quantity, and the tubes are thereby distended and enlarged. The cells are swollen, generally

opaque and granular, and often largely charged with oily particles. The large white kidney is not infrequently greatly infiltrated with fat, and then it constitutes one form of the "fatty kidney." Fatty transformation is much more frequent when the disease has arisen from cold than when it has followed from scarlatina.

In reference to the symptoms of this type of Bright's disease it cannot be too strongly insisted on that as long as the chronic inflammation of the kidneys continues to advance, or remains at its height, the quantity of urine excreted daily is far below the normal. The complete anuria which is met with in some cases of acute nephritis is, however, not observed in the chronic form. The urine, too, has a peculiar dirty tint, quite independent of bloody coloration. The dirty brown colour of the urine is increased by a further clouding which takes place as soon as it cools. When the urine is heavy and viscid from the large amount of albumen it contains, the urates, though no longer held in solution after the fluid cools, instead of falling to the bottom of the vessel, remain suspended in the fluid, and make it as thick as muddy water. The microscopic examination of the sediment shows that, apart from uric acid and its salts and other crystalline structures, the principal ingredient is urinary casts, the quantity of which in many cases is so great that every drop of the sediment may contain them in dozens. The tube-casts are of various characters—epithelial, fatty, granular, and hyaline. The longer the process has lasted the more numerous become the dark granular casts. The great mass of the casts are produced simply by deposits of spontaneously coagulable albuminoid substances derived from the albuminous urine. The dark granular casts seem to have been developed out of adherent epithelial cells that have undergone inflammatory degeneration. The broad, waxy, yellowish casts are formed principally in the straight tubes of the pyramids. The sediment also always contains white blood-corpuscles, often in considerable quantities, and, in addition, flocculent masses of granular detritus and a quantity of amorphous renal *debris*. The chemical analysis of the urine in chronic nephritis invariably reveals the presence of albumen; this is never absent at any period, and when the disease is at its height the percentage of albumen is greater than it ever is under any other circumstances. In the course of chronic inflammation of the kidneys the appearance and subsidence of dropsy appears to correspond exactly with the diminution and increase of the daily excretion of water through the kidneys; at the same time it is not to be denied that other factors exert an influence on the development and disappearance of the dropsy; that, for instance, the impoverishment of the blood caused by the draining away of its albumen promotes the increase of the dropsy, while the intercurrent diarrhoeas and the artificially provoked diaphoresis favour its disappearance.

The kidneys, when in a state of chronic inflammation, are on the one

hand unable to perform properly their natural task—the removal of the excess of water from the blood; they are also, on the other hand, incapable of efficiently performing their other important functions—the depuration of the blood from the specific constituents of the urine. It is evident from the analysis of the urine passed by patients suffering from chronic inflammation of the kidneys that the amount of urea excreted daily in this disease is far below what may be considered as the normal average excretion in healthy persons. Yet, nevertheless, uræmic attacks are by no means frequent in this disease, and only in exceedingly rare cases are they the immediate cause of death, whereas one might expect that, if retention of urea in the blood and tissues be really the cause of uræmic attacks, such attacks would necessarily and invariably occur in the course of chronic nephritis. In solving this problem it is to be borne in mind that all the factors which exert an influence on the production of urea are diminished in chronic inflammation of the kidneys. The great mass of the nitrogenous material in the blood has dwindled away, the quantity of food taken and assimilated is reduced to a minimum, and the dropsy and the debility of the patients prevent muscular exertion. It follows of necessity that the production of urea in these patients must be diminished and remain below the normal. Hence, as Bartels remarks, we should not be justified in concluding that the retention of urea in the blood and tissues does not excite uræmic attacks, because these attacks are decidedly rare in chronic nephritis, in spite of the very scanty excretion of urea through the kidneys. For who can say that even the small amount of this substance excreted with the urine does not represent fully the quantity which is produced in the tissues. But there is another circumstance which may have some influence in preventing the occurrence of uræmic attacks in chronic nephritis—viz., dropsy. A certain appreciable quantity of urea is contained in the fluid effused into the subcutaneous cellular tissue, and this quantity is increased to a very considerable percentage in the fluid effused into the serous cavities of the body. It is quite reasonable to regard these cavities as constituting a capacious reservoir for the storage of the pernicious material which cannot be excreted. The dropsy, in this respect, supplies, so to speak, a sort of natural compensation for the insufficiency of the renal functions. The importance of the dropsy as a reservoir for the pernicious urinary constituents appears established by a case narrated by Bartels, in which the most violent uræmic convulsions broke out after a profuse perspiration had been caused by a hot bath, and in that way a dropsy of considerable extent had been almost entirely reduced in a few hours.

Treatment.—Intimately connected with the treatment of Bright's disease is the question of *Prognosis*, because if the prognosis is hopelessly unfavourable there is little probability of any line of treatment being carried out with the diligence and perseverance which alone can invite

success. In Bright's first publications on the diseased conditions of the kidneys, which have since been called after him, the question—*Is this disease really curable or not?* was answered pretty decidedly in the negative. The great confusion which exists on the subject of renal diseases attended with albuminuria is a cause of the very contradictory opinions which are put forward with regard to both prognosis and treatment. He who calls every case of albuminuria that he encounters "Bright's disease," and who speaks of *chronic Bright's disease* or *chronic nephritis* when the albuminuria has lasted for more than a few weeks, may pride himself upon the number of cures he has effected. Those, on the other hand, who confine the term Bright's disease, or chronic nephritis, to the more distinctly marked cases that pursue a truly chronic course, attended by great anasarca, will scarcely give a more favourable prognosis for them than did Bright himself. Bartels' opinion is that all hope of the possibility of complete recovery must be surrendered after the affection has lasted for a certain time.

Although clinical experience certainly confirms the generally bad prognosis for these cases, yet it would be decidedly a mistake to state to patients affected with chronic parenchymatous nephritis that the disease invariably terminated fatally. For in certain of the more fortunate cases the structural changes in the kidney cease to advance, the dropsical effusions (if any existed) are absorbed, and the condition of the patient remains stationary, perhaps for months, perhaps for years; and he may be able, with proper precautions, to prolong existence in fair comfort, and even to pursue light avocations, for very considerable periods of time. The tenure of life under these circumstances is exceedingly precarious, and any imprudence in indulgence or exposure may bring life, in a few hours or days, to the verge of destruction. The protracted survivorship of some cases of chronic Bright's disease is very remarkable, and is possibly due to the circumstance that in such cases only one kidney is affected; anatomical examinations of the kidneys have also taught that although a very large part of the secreting renal structure has been destroyed, yet those portions which remained had recovered perfectly their normal microscopical features. There is also reason to believe that all the urinary tubules of the cortical substance are not affected in the same degree by the chronic inflammatory process. In judging of the possible amount of damage the kidneys may have sustained in chronic nephritis, it is well to bear in mind the fact that Nature, in constructing the body of man, has supplied with lavish hand the apparatus destined to excrete the urine—so that, even though one entire half of it be destroyed, the half that remains is capable of fulfilling the task of both. Even in the more severe cases of parenchymatous nephritis it is injudicious to give up the hope of a possibility of recovery. At the same time the prognosis of this complaint would be far more favourable if it were

recognised earlier than is, unfortunately, invariably the case. As a rule, attention is not drawn to the kidneys until the tell-tale dropsy has indicated the threatening danger—only then does the physician deem it necessary to make an examination of the urine, and he is apt to give his patient up for lost when he finds that the fluid, on the application of heat, coagulates in the test-tube to a stiff jelly. Inasmuch as the albuminuria may precede the dropsy by many months, much time may be gained for treatment by an early recognition of the nephritis before the anasarca sets in. It was before observed that among the causes which seem to operate in inducing the form of Bright's disease which we are discussing, and whose anatomical expression is the *large white kidney*, are affections which are accompanied by persistent suppurations and malarial poison, or marsh miasm. Hence, in undertaking the treatment of chronic parenchymatous nephritis the physician must endeavour to obviate any existing injurious influences which are capable of causing the disease of the kidneys or of favouring its prolongation. Examples are not wanting of the good results which have followed the cure of chronic suppurative processes by operative treatment. When the nephritis is a consequence of intermittent fever, the latter must be checked rapidly and thoroughly, and the patient, if possible, withdrawn from the influence of the malaria by removal from the miasmatic district. Complete arrest of the paroxysms of fever by sufficiently large doses of quinine is here the first requisite for the cure of renal malady. Where syphilis exists we should try to eradicate it from the system by the energetic employment of such remedies as the particular case appears to indicate.

Although the final prognosis in chronic and confirmed cases is most unfavourable, nevertheless, even in full developed cases in which the dropsy has existed for months, as long as the renal affection is not complicated by some incurable constitutional or organic disease, treatment is to be by no means regarded as impotent, provided only that both the patient and the physician employ the means at their command with sufficient patience and persistence. The expectant treatment holds out no hopes of success, as it may be positively stated that such cases never get well spontaneously. The favourable and unfavourable signs in Bright's disease have relation to the state of the skin, the duration of the disease, the degree of deviation of the urine from its normal characters, and the existence of complications. An obstinate and unvarying dryness of the skin is an unfavourable sign. The longer the disease has lasted the less is the prospect of the treatment being successful. The degree in which the urine deviates from the normal furnishes a gauge not only for the imminence of the immediate danger to life, but also for the extent of the anatomical changes which have taken place in the kidneys. Speedy death is indicated by the breaking out of pneumonia or pericarditis, by the suppression of urine, uncontrollable vomiting, or diarrhoea. The

absence of the signs just indicated may be construed as negative evidence of a favourable tendency, pointing to a stationary state, and to the probability that the final issue may be yet far distant. When we are unable to fulfil the etiological indication—the removal of the cause—the prospects of a successful result to the treatment are but poor. These are just the cases in which the disease is often not discovered until it has made considerable progress, and which are often enough only brought to the attention of the physician after dropsy has been in existence for some length of time. The search after a satisfactory means of directly treating the disease has had the result of bringing us back almost to the identical point on which Bright stood at the time of his first publication, when he regarded all chronic renal diseases that are accompanied by albuminuria as incurable. These experiments have, at least, brought us a negative profit, inasmuch as they warn us against the useless employment of all medicines and modes of treatment that are directly weakening, or that derange the digestive organs of the patients, who are usually already anæmic. It is important to get patients with chronic Bright's disease to take iron in some form, as the best safeguard against the profound anæmia, which is a fertile source of danger to sufferers from chronic renal degeneration. The non-astringent salts of iron, administered in infusion of calumba, are occasionally useful—from three to five grains in an ounce of the infusion being taken three times daily, an hour after food. In cases where severe headache seems to result from the administration of this form of the drug, it may be given as a citrate, in combination with free citric acid and citrate of ammonia. The following is recommended by Dr. Carter* as a very nice form:—Citrate of iron, gr. 5; citric acid, gr. 5; liq. ammoniæ citratis, 4 oz.; syrupi aurantii, 3 iv.; and either water or infusion of calumba to 8 oz.—one ounce three times a day, an hour after meals; or the same preparation, the citrate, may be given in the effervescing form. Forty grains of it and 80 grains of citric acid dissolved in 8 oz. of water, sweetened with syrup of oranges or other flavouring ingredient, make a palatable mixture, of which an ounce may be given three times daily, with 15 grains of bicarbonate of potash in a state of effervescence. It is best to drop the powder—the fifteen grains of the bicarbonate—into the mixture.

With the view of controlling the quantity of albumen lost in the urine, the drugs which contain tannic acid were tried by Bright, and, especially since Frerichs recommended them, have been very generally employed. The opinion of most observers is that they exercise no influence upon the excretion of albumen, while in some cases they excite serious gastric disturbance. The treatment of chronic nephritis by drastic purgatives is found to have nothing to recommend it; any reduction of the dropsy thus effected is far outweighed by the injurious influence they exert on

* *Clinical Reports on Renal and Urinary Diseases.* 1878. P. 189.

the already anæmic patients. The methodical employment of diaphoresis seems to offer the only expectation of curative results on the processes of the disease, but to justify such expectation it must be carried out systematically and with obstinate pertinacity. The patient should be habitually clothed in flannel, both limbs and trunk, diligent frictions of the surface of the body; and the other measures for procuring diaphoresis which were alluded to in the treatment of acute nephritis must be continued without interruption until the urine shows signs of abatement of the inflammatory process in the kidneys. Bright held strict confinement to bed to be the most reliable of all the methods at our command for the maintenance of the proper degree of activity of the skin, and without this he thought there was no prospect of curing the disease in our climate. Should opportunity offer, a voyage to the West Indies and a residence on one of the healthier islands there would be likely to effect a great change in the constitution, principally on account of the action upon the pores of the skin. The effect of jaborandi and pilocarpine as diaphoretics deserve more extensive trials than they have yet obtained. In established cases of Bright's disease the diuretic salts have been found to do little or no good. Immermann has recently strongly advised the acetate of potash in large doses (five to ten grammes a day). The diet should be regulated so as to suit the particular state of the digestive organs. The consumption of milk or buttermilk should be as large as the person can tolerate; both of these substances have undoubtedly a beneficial diuretic action. Care must be taken to provide the patient with the most abundant practicable compensation for the loss of albuminous matter to which he is subjected. This is a problem often rendered most difficult to solve by the dislike the patients themselves so often evince for a meat diet or animal food in any shape. With the assistance of hydrochloric acid and pepsin, some amount of the concentrated meat essences may be managed to be assimilated with care and patience. The stronger wines and all spirits agree, as a rule, badly, and should be prohibited, unless under special circumstances.

The dropsy of the limbs and genitals sometimes requires mechanical relief in order to prevent rupture of the skin. In no other renal affection does it attain to such a grade. Bartels observes that in no other disease of the kidneys has he so often observed extreme anasarca as in the chronic parenchymatous inflammation. Niemeyer mentions the case of a patient who doubled his weight in eight weeks from this cause. The extreme dropsical swelling, from the strain which it exerts, may, as was before mentioned, be the cause of inflammation and superficial gangrene of the skin, especially of the scrotum and labia majora. The time, the place, and the means of affording exit to the accumulating serum are all subjects of anxious consideration. Many do not look with favour upon the practice of making scarifications for the relief of the anasarca. Thus

Bartels observes that the dropsy may be subdued by other means with more durable results; cuts made into œdematous skin readily become starting-points for erysipelas and phlegmonous inflammations, and besides they are open to the objection of wetting the bed, and thus causing all kinds of inconvenience and harm. Sir W. Gull's^a experience, as expressed in the Clinical Society of London, is that in the œdema of heart disease puncture answers admirably, but in that due to renal disease it is of less benefit, if it does not positively do harm. Murchison^b observes that there is this objection to acupuncture in renal dropsy—namely, that the serum being loaded with urea or other impurities is apt to excite inflammation and abscesses at the seats of puncture, which are often followed by erysipelas and sloughing. Under such circumstances he thinks preferable the operation originally recommended many years ago by Dr. Mead. This consists in making an incision about an inch in length in either leg, two finger-breadths above and behind the inner ankle, through the true skin into the subjacent areolar tissue. The incision of Mead is approved of by Dr. George Johnson,^c who writes as follows:—"When other means fail to remove the dropsy, when the anasarca distension of the legs is increasing and causing pain and incipient erythematous inflammation, or when the breathing is becoming impeded by the accumulation of water within the abdomen or the chest, or by an œdematous condition of the lungs, prompt, decided, and sometimes permanent relief may be afforded by allowing the water to escape through an incision in the skin about half an inch long, just above either the outer or the inner ankle of each leg; the incision must be deep enough to enter the areolar tissue beneath the skin."

The operation of puncturing the skin is sometimes painful enough to particular patients, and it is well to bear in mind that this annoyance may be got rid of by using a small spring scarifier such as that with a single row of lancets used for cupping the temporal region. Dr. Johnson gives an illustration of this fact:—A dropsical patient had his legs acupunctured by the house physician, and cried out with the pain caused by the needle punctures. A few days afterwards, the punctures having ceased to discharge, while the dropsical swelling was but little reduced, an incision was made into each leg with a spring lancet. He declared that he scarcely felt the cuts; and the incisions discharged so freely that the dropsy was for a time completely removed. After the dropsical legs have been punctured, the folded sheet and mackintosh, placed beneath to receive the serous discharge, should be frequently renewed and kept clean. The liquid quickly decomposes and becomes ammoniacal, and in this state it may irritate and inflame the skin. Cleanliness is as essential

^a Brit. Med. Jour. 11th March, 1871. P. 254.

^b Op. cit. 25th May, 1872. P. 548.

^c Lectures on Bright's Disease. 1873. P. 143.

for safety as for comfort. Any inflammatory redness about the wounds may usually be removed quickly by the application of a lead lotion. It is true that severe inflammation and sloughing have sometimes followed incisions or punctures in anasaruous legs, but this may, and often does, occur from over-distension of the skin, or from the mere pressure of the heavy dropsical limbs upon the bed. The result of Dr. George Johnson's^a experience is that inflammation of anasaruous legs has been as often subdued as provoked by acupuncture or incision; that inflammation is much less likely to follow incisions in cases of renal than of cardiac dropsy, when the circulation is much impeded by valvular disease; and that incisions made with the spring scarifier are as safe as acupuncture, and much less painful. The capillary trocars invented by Dr. Southey form a convenient mode of withdrawing the anasaruous fluid. By using these instruments the fluid is kept from contact with the skin; one may be introduced into each leg, and as much as over 140 oz. may be drained away by a single trocar in twenty-four hours.^b

AUSCULTATION OF THE SKIN FOR CHIRURGICAL PURPOSES.—DERMATOPHONY.

C. HUETER (*Centralblatt f. die medicinische Wissenschaften*, 1878, Nos. 51 and 52), having found that the vascular bruit of the pulp of the finger was clearly audible upon a microphone, constructed a cheap and efficient "dermatophone," by stretching a piece of gutta-percha over the open end of a binaural stethoscope (Votolini's, but Camman's might do). On placing this plate over certain vascular parts, such as the finger-tips, malar eminence, eyeball, &c., a peculiar sound, varying somewhat in pitch in different parts, is heard. This sound is the normal bruit of the subcutaneous capillaries and smaller vessels. If the hand be rendered bloodless by means of an Esmarch's band, the vascular bruit can no longer be heard in the finger-tips. In acute cutaneous inflammation (furuncle, paronychia), a louder but deeper note is heard. In a case of partial stasis of blood in the foot, caused by a too tightly applied dressing for fracture of the thigh, the toes being dusky and cool, the vascular sound was weaker and hardly perceptible; but after loosening of the bandage, and a restoration of heat to the foot, it became louder, then normal. The instrument is also applicable to the muscles and tendons—myophony and tendophony. Applied to superficial bones while they are percussed (with a hammer or whalebone), according to Lücke's method, a distinct sound is heard. The author anticipates that these applications of auscultation will prove of help in medical and surgical diagnosis.—*Archives of Medicine*, Vol. I., No. 2, April, 1879.

^a Lectures on Bright's Disease. 1873. P. 145.

^b Carter. Urinary and Renal Diseases. 1878. P. 87.

SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P.

VITAL STATISTICS

Of Eight Large Towns in Ireland, for Four Weeks ending Saturday, March 22, 1879.

Towns	Population in 1871	Births Registered	Deaths Registered	DEATHS FROM ZYMOTIC DISEASES							Annual Rate of Mortality per 1,000 Inhabitants
				Smallpox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhoea	
Dublin, -	314,666	739	909	62	12	20	4	7	23	7	37·6
Belfast, -	182,082	543	513	—	4	8	1	21	23	18	36·5
Cork, -	91,965	208?	173?	—	—	—	—	4	6	2	25·6!
Limerick, -	44,209	105	138	—	—	—	—	10	3	9	40·5
Derry, -	30,884	94	62	—	3	—	—	—	—	—	26·3
Waterford, -	30,626	39?	64?	—	—	—	—	—	1	—	36·3!
Galway, -	19,692	39	54	—	—	—	—	—	3	1	35·8
Sligo, -	17,285	28	25	—	—	—	—	—	—	1	18·8

Remarks.

Although considerably less than in the previous four weeks, the mortality was still generally high, especially in Limerick, Dublin, Belfast, Waterford, and Galway. The returns for Cork in the week ending Saturday, March 15, are incomplete; and those for Waterford in the week ending Saturday, March 8, are wanting. The death-rate of the four weeks was 28·4 per 1,000 of the population annually in London, 19·0 in Edinburgh, and 25·1 in Glasgow. Within the municipal boundary of Dublin it was 38·5 per 1,000, and, omitting the deaths of persons admitted into public institutions from localities outside the district, the death-rate of the Dublin registration district was 36·1. Considering the very inclement weather which prevailed in Scotland, the mortality in Edinburgh was singularly low. In Dublin, zymotic diseases caused 155 deaths, or 17·1 per cent. of the total mortality. Although this was an improvement on the preceding four weeks, when the deaths from zymotics numbered 243, these affections were more fatal than they

had been on the average in the corresponding period of the previous ten years (129·0 deaths). Smallpox and fever show a decided decline. The deaths from fever were only one-half as numerous as those registered in the preceding four weeks (46). Of the 23 fever deaths, 5 were ascribed to typhus, and 18 to typhoid. At the close of the period, 135 smallpox patients were under treatment in the Dublin hospitals, compared with 177, 188, 122, 86, and 47 at the close of the five preceding periods respectively. Whooping-cough and diarrhoea continue to be unusually fatal in Belfast and Limerick. Milder weather caused a decided diminution in the deaths from affections of the organs of respiration in Dublin. The deaths were 240, or 26·4 per cent. of all the deaths, and included 189 from bronchitis, and 36 from pneumonia. The ten-years' averages were—respiratory affections generally, 195·3; bronchitis, 153·1; and pneumonia, 24·0 deaths respectively.

METEOROLOGY.

Abstract of Observations made at Dublin, Lat. 53° 20' N., Long. 6° 15' W., for the Month of March, 1879.

Mean Height of Barometer,	-	-	-	29·948 inches.
Maximal Height of Barometer (on 7th at 11 p.m.),	-	-	-	30·574 „
Minimal Height of Barometer (on 28th at 9 p.m.),	-	-	-	29·281 „
Mean Dry-bulb Temperature,	-	-	-	41·9°
Mean Wet-bulb Temperature,	-	-	-	39·7°
Mean Dew-point Temperature,	-	-	-	36·9°
Mean Elastic Force (Tension) of Aqueous Vapour,	-	-	-	·222 inch.
Mean Humidity,	-	-	-	84·0 per cent.
Highest Temperature in Shade (on 8th),	-	-	-	56·2°
Lowest Temperature in Shade (on 1st),	-	-	-	31·5°
Lowest Temperature on Grass (Radiation) (on 13th),	-	-	-	27·5°
Mean Amount of Cloud,	-	-	-	63·7 per cent.
Rainfall (on 16 days),	-	-	-	1·827 inches.
General Direction of Wind,	-	-	-	W. and E.

Remarks.

This was an averagely fine March—the first half of the month being comparatively dry and mild with prevalent S.W. to W. winds; the latter half being dull and cold with keen E. winds and frequent falls of sleet, snow, and hail. The mean temperature was about one degree Fahrenheit below the average of the previous thirteen years. During the first five days atmospherical depressions were passing in rapid succession north-eastwards across Scotland, the North Sea, and Scandinavia; so that strong S.W. to W. winds were felt in Ireland and Scotland. On the 6th an anticyclone spread over the United Kingdom from the southward,

and the weather became calm and bright with a large diurnal range of temperature. On Sunday, the 9th, this fine weather system broke up, conditions becoming unsettled with sudden and extreme alternations of temperature. On the afternoon of the 12th the relative humidity in Dublin fell to 57 per cent., and at night frost occurred with a clear sky and N. wind. After the 18th barometrical gradients for easterly or polar winds became fully established in the British Isles. These E. winds were portion of the circulation in an anticyclone over Scandinavia and Russia, in which regions very intense cold prevailed. In Dublin the thermometer never rose to 40° in the shade from the afternoon of the 22nd to the evening of the 28th. During this period the sky remained almost completely overcast, the wind blew strongly from E.N.E., E., or E.S.E., and snow and hail fell in frequent showers. The mean temperature of the week ending Saturday, the 29th, was 37·1°, or 7·9° below that of the week ending Saturday, the 16th. On the evening of the 28th the wind drew round to S.W. after a severe, though transitory, S.S.E. gale, and the weather became mild and spring-like. Owing to the clouded state of the sky during the cold weather no very low temperatures or severe frosts were recorded—in fact, the shade thermometer fell to 32° on only three occasions. A faint aurora was seen at 11 p.m. of the 10th. A solar halo was noted at 9 a.m. of the 13th. A lunar corona appeared at 7 30 p.m. of the 6th. Sleet fell on the 16th, 17th, and 29th; snow on the 24th and three following days; hail on the 23rd and six following days. There was a dense vapour fog on the night of the 18th, and heavy dews were remarked on the 7th, 8th, and 18th.

CREASOTE IN PHTHISIS.

DR. BONNEFONTAINE (*Union Méd.*, March 11, 1879) has found that consumptive patients who are rather fanciful concerning their food and medicine will easily take creasote in the shape of Dartais' capsules. These are very small globules, containing each about five centigrammes of creasote, and quite tasteless. The drug must be taken three times a day, before every meal, in doses of three globules each time, and followed by a cup of chocolate or milk, a glass of wine, or some soup.—*Lond. Med. Record*, April 15, 1879.

COMPRESSED AIR-BATHS IN WHOOPING-COUGH.

M. MOUTARD-MARTIN says (*Union Méd.*, March 11, 1879) that compressed air-baths are very efficient in every stage of whooping-cough. He has treated three patients, aged respectively seven, twelve, and fourteen, with compressed air in the incipient stages of the affection, and in every case it assumed a mild form, and did not last long.—*Lond. Med. Record*, April 15, 1879.

PERISCOPE.

Edited by G. F. DUFFEY, M.D., F.K.Q.C.P.

TREATMENT OF PNEUMONIA.

WE extract the following from some notes on the treatment of pneumonia in Bellevue Hospital, New York, published in the *Medical Record* of that city:—"Hospital pneumonias form a class of diseases especially liable to elude statistics regarding fatality and treatment. . . . The majority of the patients brought to Bellevue trace their pneumonia to exposure. Owing to the previous bad habits and bad condition of the patients, as well as to the frequent virulence of the disease, the treatment of many cases of pneumonia soon resolves itself into the treatment of exhaustion, œdema of the lungs, and a nearly moribund condition. How to carry a patient through conditions of this kind, how much stimulants, and of what kind, are matters that text-books do not give very definite accounts of, and experience has generally to become the teacher. Many cases that come into the hospital have a record like the following:—The patient is a tolerably strong man, in the third day of the fever. He has a temperature of 104°, respiration 50. His pulse is very good, and he feels pretty comfortable. He is given milk and eggs, and gr. xv. of quinine. This is in the morning. In the afternoon he is weaker, his face is a little blue, he breathes faster. On listening to the lungs, moist râles, fine and coarse, are heard. He is beginning to have œdema. He is at once dry-cupped for fifteen or twenty minutes, during which time 150 cups are put on. The œdema has now disappeared. He is ordered m̄x. of tr. digitalis every three hours, and ʒ ss. of whiskey every two hours, with milk and eggs. He continues better for some hours. Towards evening the œdema again shows itself. He is again cupped, and gr. x. ammon. carb. is ordered every two hours, alternating with the whiskey. Again the œdema clears up. In addition, a can of oxygen is ordered for the night, and the patient inhales it for fifteen minutes in every hour. This relieves his dyspnœa. But towards morning the cyanosis and œdema again appear. The cups are applied again, and the whiskey ordered, ʒ ss. every hour, alternating with the ammonia, gr. x., every hour. By these measures he is carried through the night, and in the morning is easier. Nourishment, in the form of milk, is still kept up. He is not allowed to sleep continuously, for during sleep the œdema comes on. By such fighting as this, the greatest reliance being placed on whiskey, milk, and dry cups, a patient is occasionally brought through. If, on the following day, he is still worse, the resources in the way of

stimulants are not exhausted. Other forms of ammonia are used. Hypodermic injections of camphor dissolved in sweet oil are given every three hours in four-grain doses. If the patient has persistent œdema and a full pulse, venesection is tried, and is invaluable when digitalis and cups no longer avail. The oxygen cannot be pushed too much, as it causes unconsciousness. Hypodermic injections of ether to the amount of one or two drachms sometimes bring up the pulse. Teaspoonful doses of champagne every five minutes will help to tide a crisis. There is a limit to stimulation, of course. When $\frac{3}{4}$ ss. of whiskey every half hour has no effect the patient will die. There is no use in increasing the amount unless it is desired to preserve the stomach afterwards. In the case of a man to whom $\frac{3}{4}$ ss. q. $\frac{1}{4}$ h. was given for several hours, the stomach was found at the autopsy to be considerably hardened by the alcohol and to smell strongly of it. In spite of all efforts, most of the cases in which œdema of the lungs occurs go on from bad to worse. The extremities become cold and wet with perspiration; hot bottles applied to them and to the breast, mustard to the epigastrium, accomplish only temporary good, the patient has long been stupid; he now loses consciousness, his breath is slow and laboured, the air-passages are full of serum; gradually the respirations grow slower and slower, and finally stop. The patient dies. At the autopsy part of the lung will be found to have reached the stage of gray hepatisation."

OBSTINATE SINGULTUS CURED BY MURIATE OF PILOCARPINE.

DR. ORTILLE reports a case of persistent singultus, due to cerebral embolism, which proved utterly rebellious to all the usual methods of treatment. As the singultus persisted even during the sleep produced by morphine injections, and the strength of the patient was becoming greatly reduced, a hypodermic injection of half a grain of pilocarpine was at last administered. This produced abundant perspiration and salivation, and the hiccough ceased at once.—*Allg. Med. Cent. Zeit.*, and *N. Y. Med. Record*.

BELLADONNA IN THE TREATMENT OF INTESTINAL OBSTRUCTION.

DR. NORMAN KER (*Le Praticien*, March 3, 1879) reports five cases of intestinal obstruction which have been cured by the administration of large doses of belladonna. The treatment consisted in giving one or two grains of belladonna every hour, together with opiate fomentations to the belly and warm applications. Nearly all the patients were in a dangerous condition, but were entirely cured, the remedy taking effect in six to nine hours. One patient took sixteen grains of the extract. The author gives no precise account of the cause of the obstruction.—*The Practitioner*, April, 1879.

JABORANDI IN THE TREATMENT OF PUERPERAL ALBUMINURIA.

DR. FORDYCE BARKER expresses the conviction, after the clinical observation of many cases, that the utility of jaborandi in the treatment of puerperal albuminuria is more than doubtful, and that, after puerperal convulsions; its depressing influence and action, which is continuous and exhausting, prevents sleep and the repose of the nervous system, and thus renders it in these cases an unsafe and dangerous remedy.—*N. Y. Med. Record*, March 1.

CORYZA.

DR. RUDOLPHO RUDOLPHI recommends the use of eucalyptus globulus for the rapid cure of *acute* coryza, or cold in the head. He has found, by numerous trials on himself and patients, that after chewing a few of the dried leaves and slowly swallowing the saliva, the affection is promptly relieved, often disappearing in the course of half an hour. The remedy is useful in acute cases only.—*Gazz. Med. Ital. Lombardia*, January, 1879, and *N. Y. Med. Record*.

POISONING FROM THE EXTERNAL APPLICATION OF TOBACCO.

DR. O'NEILL (*Lancet*, March 1, 1879) was called to see a robust woman of forty, who was represented as bleeding profusely from a wound on the leg caused by an accidental blow. The patient suffered from varicose veins. On arrival he found that the bleeding had stopped, but the patient had fainted, and had been only revived with great difficulty. She seemed to be extremely prostrated, having a pulse scarcely to be felt, and a pale, cold skin, wet with profuse, clammy perspiration. The pupils of the eyes were dilated, and in a faint whisper she complained of dizziness, dimness of sight, and confusion of thought. The patient suffered from pain in the abdomen, and from constant nausea and vomiting; and on removing the bandages and examining the leg, a handful of chopped wet tobacco was found firmly pressed on the wounds, which were deeply stained by it. The surrounding skin was also stained by the tobacco, but in a slighter degree. Pinches of tobacco had been applied on previous occasions to stop slight bleeding, and after each application the patient had felt rather unwell, but had failed to connect the indisposition with the tobacco. The tobacco was immediately removed, the leg was well cleansed, stimulants were given and a mixture containing strychnia, which, according to the Rev. Professor Haughton, is the true antidote of tobacco. Under this treatment the patient gradually improved, but did not entirely recover for several days. Commenting upon this case Dr. O'Neill says the symptoms of poisoning were well marked, and are worthy of note when the mode of introduction of the poison into the system and the rarity of such a mode of introduction are taken into consideration. In Lincolnshire tobacco is occasionally

used as a domestic remedy for stopping bleeding. In the case under consideration hæmorrhage was arrested; but, as tobacco does not contain any astringent substance, the handful of chopped leaves acted in all probability merely as a compress, or as a means of entangling the blood, and thus allowing it to coagulate on escaping from the injured vessel. The urgent symptoms in this case very soon set in, for the patient was seen about three-quarters of an hour after the application of the tobacco, and yet she had fainted in the interval. Absorption therefore must have been rapid, nearly as rapid as if the tobacco juice had been injected subcutaneously.—*The Practitioner*, April, 1879.

ANEURISM OF THE LEFT VENTRICLE.

AT a recent meeting of the Société des Sciences Médicales a very interesting specimen of an aneurism of the left ventricle was presented. The patient had always been healthy, but much addicted to drinking. In April, 1878, the first symptoms of the subsequent affection appeared—*anorexia*, migrating pains in the groins, and rapid loss of flesh. Subsequently he began to vomit his food, either at night or the next morning. On examination, a hard tumour, which occupied about four square inches, was found in the epigastric region. The patient looked cachectic, but no other disease or trouble could be discovered at the time. In November a very small amount of fluctuation could be felt in the lower portions of the abdomen. In December the tumour could no longer be felt, the patient vomited his food about an hour after taking it, and died on the next day. At the necropsy it was found that the whole of the stomach was filled with alimentary matter. The small curvature was entirely occupied by a hard, fibrous neoplasm, which surrounded the pylorus, constricting it to a considerable extent. This tumour was attached by adhesions to the posterior walls of the abdomen, the pancreas, and spleen; all the parts covered by it were hard. The most interesting object, however, was the heart, which, although of normal weight and size, showed on the outside a tumour of the size of a nut, which, on an incision being made, proved to be an aneurism of the left ventricle. Its walls were rugged, the whole of it was calcified, and blood-clots and fibrin were found between the partitions of the inner walls. An embolus originating from one of the above-mentioned clots must in all probability have been the cause of death. The diagnosis—*alcoholic cirrhosis of the liver*—had been made previous to the patient's decease.—*Lyon Med.*, January 26, 1879, and *London Med. Record*, April 15, 1879.

ERRATUM.

In the "Transactions of the Medical Society of the College of Physicians," in the April number of the Journal, page 322, line 7 from the bottom of the page, the word "glycerine" should be "alizarin."

THE DUBLIN JOURNAL

OF

MEDICAL SCIENCE

JUNE 2, 1879.

THE ROYAL
SOCIETY FOR
MEDICAL
OBSERVATION

PART I.

ORIGINAL COMMUNICATIONS.

ART. XVIII.—*Laryngeal Polypus treated by Laryngotomy and subsequent Evulsion and Cauterisation.* By KENDAL FRANKS, M.D., Univ. Dub.; F.R.C.S.I.; Surgeon to the Adelaide Hospital; Surgeon to the Throat and Ear Hospital.

SINCE the introduction of the laryngoscope the diagnosis and treatment of growths in the larynx has received a great deal of attention. We no longer work in the dark, nor are we obliged to rest an opinion as to the existence of abnormal conditions of the larynx upon symptoms which, unaided, are quite inadequate to justify a conclusion. We are not now satisfied to wait till a fatal termination reveals the true nature of the disease. In the present day we possess the means of arriving at an accurate diagnosis. We can see imaged in the laryngeal mirror the cause of the patient's sufferings, the symptoms are clearly explained by sight, and, more than all, an appropriate treatment can now be employed which the ante-laryngoscopic era forbade. It is now just twenty years since a laryngeal polypus was seen for the first time in the mirror by Czermak. Since then the subject has attracted so much attention that, in the words of Von Ziemssen, "the literature of the laryngoscopic period abounds in recorded observations to a degree almost oppressive." It is not to add to this oppression that I record the case which I am about to detail, but in order to draw attention to a method of treatment which is particularly applicable

under certain circumstances, but which has, however, seldom been employed.

Laryngeal growths in general may be treated by intra- or extra-laryngeal methods. Intra-laryngeal operations—such as evulsion with forceps, snare, &c., ligature, excision or cauterisation—being far less dangerous to life than any of the extra-laryngeal methods, one of these operations should be preferred when equal success can be thus obtained.

Fortunately the greater proportion of cases are suitable for treatment *per vias naturales*. But this is not always the case, for the growth situated in the cavity of the larynx may be so large, and have such an extended attachment, that it would be impossible to remove it all through the mouth, as in the case^a brought before the Clinical Society of London on February 22, 1878, in which Mr. Lister performed thyrotomy, and removed both vocal cords, together with the ventricular bands and the neighbouring portions of mucous membrane. Or the polypus may be situated in the subglottic region, in which case it may be almost impossible to grasp it from above—especially if the growth be in front, below the anterior attachments of the vocal cords. In such cases endolaryngeal measures are unavailing: we have no choice but to have recourse to an extra-laryngeal operation—that is, if the growth is in any way a source of danger to the patient.

Of extra-laryngeal methods of removing growths from the larynx, three are recognised:—

I. *Supra-thyroid Laryngotomy*, or division of the thyro-hyoid membrane, is only required when the growth is situated at the upper orifice of the larynx, and cannot be removed *per oram*.

II. *Thyrotomy*, or division of the thyroid cartilage. “This operation,” says Mackenzie,^b “may be required for the removal of large growths in the cavity of the larynx, causing great dyspnoea or dysphagia, which cannot be removed with the aid of the laryngoscope; or for the evulsion of growths in the subglottic region, which cannot be extirpated by indirect laryngotomy (through the crico-thyroid membrane).” This operation is one of no small danger to life, and the results of which are so doubtful, both as regards recovery of voice and recurrence of growth, that it should only be had recourse to when all other means are inapplicable, and when the safety of the patient demands it. Professor

^a British Medical Journal. March 2, 1878.

^b Growths in the Larynx. Morell Mackenzie. 1871.

Bruns, of Tübingen, has lately given statistics to show that thyrotomy has been followed by complete, or nearly complete, restoration of voice in only 46 per cent. of 39 selected cases, whilst recurrence of the growth took place in about 54 per cent.^a

III. *Infra-thyroid Laryngotomy* is required when the growth is situated below the vocal cords, or in the upper part of the trachea, and cannot easily be removed by endo-laryngeal methods. It is exceedingly doubtful whether the removal of such growths should be attempted by the mouth, as it can only be thus effected "with great difficulty, and after several months' close attendance." Moreover, the whole of the growth can rarely be got away in this manner—hence recurrence is more probable, and the voice frequently remains affected. Laryngotomy obviates these difficulties, and, as may be seen in the case I am about to describe, complete restoration of voice may be expected. "It is very remarkable," says Morell Mackenzie,^b "that this operation has not been more frequently performed;" for up to 1871, when he made this observation, but two cases had been recorded—one by Dr. Burow, and one by himself.

CASE.—The subject of the present communication was a woman named A. S., aged fifty-five, who applied for relief at the Throat Dispensary of the Adelaide Hospital, on October 7th, 1878. She has been a widow for thirty years, during the latter fifteen of which she followed the occupation of a cook. Having been obliged, on account of the distress she experienced in her throat, to give up work, she left her last situation and came to live with her daughter in Lower Mercer-street, about nine months previous to her admission into hospital. She stated she had been hoarse for twenty years, but otherwise she enjoyed perfect health. About nine months ago she experienced difficulty in swallowing—even the saliva causing some pain—and often had a "choking feeling," as though some foreign body were in the larynx. She completely lost her voice. These symptoms increasing, she sought advice, and was treated by a mustard poultice placed over the throat. She thought that this partially removed the "choking" sensation. The symptoms, however, became aggravated, till at last she could hardly lie down at night—suffocation in this position sometimes seeming imminent. Accordingly she used to be propped up in bed, and on several occasions she was obliged, she stated, to rush into the open air "to catch her breath." When she coughed there was scarcely any expectoration. The daughter now compelled her to attend the Throat Dispensary at the Adelaide,

^a British Medical Journal. March 2, 1878.

^b Op. cit.

which she did for the first time on October 7th, 1878. She was examined by Dr. Walter G. Smith, who kindly showed the case to me. She was a dark, sallow woman, of middle height, and fifty-five years of age. She could not speak above a whisper. She complained of no pain, but had a "husky heat" in the throat, with a constant feeling of dryness. She had an habitual short cough, and was always trying to clear phlegm from the larynx, but could only get up a little stringy, glairy mucus. On laryngeal examination, a polypus was seen almost to fill up the rima glottidis. It was pedunculated, but had a rather broad attachment to the anterior half of the left vocal cord. It moved freely on inspiration and expiration, flapping up between the cords. Whenever the cords were approximated the polypus was hid from view, being attached below them. It appeared to be fibro-cellular in structure, closely resembling similar growths in the nose, and had small vessels ramifying over its surface. [See Plate I., Fig. 1.]

Attempts to reach the polypus were now made. In the first instance I tried Gibbs' laryngeal snare, but being unguarded, and bending at the first contact with the larynx, it only succeeded in nipping a small piece of mucous membrane. Dr. Smith and I then, on several occasions, made attempts with various kinds of forceps, but, though apparently often touching the growth, we found it impossible to grasp it; for every time the forceps entered the larynx, the larynx closed, the cords approximated, and the growth disappeared from view. Then I hoped that Stoerks' guarded guillotine might succeed by passing it into the larynx, keeping it *in situ* till the cords separated on an inspiration, and then suddenly slipping it down between them. This frequently succeeded in getting the snare past the rima, but still, in spite of numerous attempts, every effort to get the wire noose round the polypus proved abortive. These endo-laryngeal attempts were continued at intervals of two or three days for more than a month, and then it became evident that other means would have to be resorted to in order to eradicate the growth. Having made a sketch of the laryngeal image as seen in the mirror, I showed it to my cousin, Dr. Philip C. Smyly. He gave it as his opinion that laryngotomy was the proper means to remove the growth. In this view Dr. W. G. Smith fully concurred, and, having obtained the approval of my colleagues and the consent of the patient, on November 27th laryngotomy was performed, ether having been previously administered by my colleague, Dr. Barton. The violent spasms of coughing which followed on the larynx being opened prevented the complete removal of the crico-thyroid membrane. The polypus could not be seen, and, as the coughing continued for some time, my colleagues advised me to postpone any further attempt at its removal for some days. A large-sized, plain tracheotomy tube was accordingly secured in the larynx, and the patient put to bed in a small ward, in which a constant

Plate 1.

FIG I

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FIG II.

with the nasal Franks

LARYNGEAL POLYPUS

moist temperature of 64° F. was maintained. Some of the senior students of the hospital took it in turns to watch her day and night, and paid unremitting attention to the case, which was placed more directly under the care of Mr. William F. Elsner. The discharge through the tube was large in quantity, extremely viscid, and tenacious, so that the inner tube had to be cleaned out every half hour. The next day she seemed to be going on well, though she coughed a good deal. In the evening, at 6 p.m., she was flushed and the skin hot, so I examined the chest, and found dulness over the base of the right lung, which had not previously been discovered, though she had been carefully examined. The pulse was 116, temperature 102·8°, and respiration 30. The next morning Dr. Smith found the same condition, the pulse being 110, temperature 102·2°, and respiration 30, and we feared the occurrence of pneumonia. However, the temperature gradually fell, the dulness cleared off, and on December 4th the lung was normal, the pulse 90, respiration 23, and temperature 99·6°. It was now decided to attempt the extraction of the polypus.

The patient, being anæsthetised, was placed in such a position that the strong light of the window should fall directly on the throat. I then made several attempts to grasp the polypus by passing a forceps, the end of which was bent at an angle of about 120° through the crico-thyroid space. On the 7th December one of these succeeded in tearing off a small piece, about the size of a split pea. Every attempt to eradicate the rest of it proved futile, though different kinds of forceps were used; and I then determined to try to effect my object with caustic. I had a bulb of silver fastened on to the end of an ordinary laryngeal bougie, which, being made of pliable metal, could be bent readily at any angle required. I bent it about three-quarters of an inch from the end to a right angle, and, having fused some nitrate of silver on to the bulb, I passed it through the crico-thyroid space. Directing the bulb upwards and slightly to the left side, I depressed the handle so as to bring the end of the instrument as much forward as possible. By this means the nitrate of silver could reach the polypus situated beneath the anterior angle of the rima. The instrument being rapidly removed, violent coughing supervened, which lasted for some minutes. The patient subsequently complained only of a burning in the throat. This was on the 14th of December. To prevent this caustic holder cauterising more than was required, the bulb was enveloped in a thin layer of cotton-wool, save at the extreme end. The next day the voice for the first time became audible, though very rough and hoarse. The pain had completely subsided.

On examination with the laryngoscope, the polypus appeared white and opaque. Four days subsequently the nitrate of silver was applied as before, and was followed by the same symptoms. On the 20th, two days after the second application, the voice had greatly improved, the

patient herself observing that she could "shout grand." All difficulty of swallowing had disappeared, she had no "choking" sensations, and could breathe with natural ease. Her appetite had materially improved. On the 25th, a laryngeal examination was made; the growth appeared greatly reduced in size, and seemed to be sloughing. The caustic was applied to the growth freely. Next day the voice was completely gone, and she complained much of the burning in the throat. However, in a few days the voice returned gradually, and soon became stronger than it had hitherto been. She was greatly pleased with the improvement, and informed the bystanders: "I could call a cab now in the street, if I only had the money to pay for it"—a hint which subsequently was put in a less ambiguous manner. No application was made after this till Jan. 15, when the charged laryngeal porte-caustique was used, as on previous occasions, and was repeated every second day till Feb. 1, when, on a careful examination of the larynx being made, nothing abnormal was seen except a slight thickening of the left vocal cord anteriorly. The polypus had completely disappeared. Fig. 2—representing the condition of the larynx at this time—was taken from a sketch which I then made. The voice had become quite strong, never failed her, and, for a woman fifty-five years of age, was very good. Deeming further interference unnecessary, I now removed the tracheotomy tube—one of Durham's, which caused her less distress than any other. A conical plug of lint, dipped in carbolic glycerine, was placed in the wound, and a strip of plaster made all secure. In a few days the wound had completely closed, leaving nothing but a slight scar to tell of the operation. She shortly after went to the Convalescent Home at Stillorgan. Dr. Walter G. Smith, who took a great interest in the case, and with whose assistance most of the treatment had been carried out, examined the case quite recently, more than three months after the external wound had been closed, and kindly sent me the following results:—

" May 9th, 1879.

" DEAR FRANKS,

" I examined Mrs. S. with the laryngoscope several times since the closure of the tracheotomy wound; and on the last occasion, about a fortnight ago, I could perceive nothing abnormal in the larynx beyond a slight thickening of the left vocal cord. All traces of the polypus, as such, had disappeared, and her voice was perfectly restored.

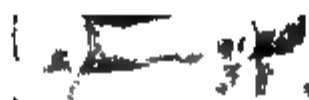
" Yours faithfully,

" WALTER G. SMITH."

Whilst the question as to the advisability of performing laryngotomy in this case was still *sub judice*, a case which six years ago had come under the notice of Dr. Coppinger, Surgeon to the Mater Misericordiæ Hospital, and with the particulars of which he has

Plate II.

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Drawn by Ker. de. Franks

Lithographed by John Filchner Dubler

LARYNGEAL POLYPS

kindly furnished me, convinced me as to the danger of delay. The patient was a healthy-looking man, a resident in county Wicklow, who came up to Dublin complaining of dyspnœa, which he had suffered from for three years. He had no aphonia, but on exertion there was slight laryngeal stridor. He complained of severe attacks of loss of breath having come on on two or three occasions whilst going up hills. He mentioned to Dr. Hughes, to whom he had been sent from Wicklow, that once whilst leading a van he had fallen down from sudden dyspnœa. In a short time he recovered. On his journey up to town he had several attacks in the train. Except the slight laryngeal stridor, he showed no other symptoms at the dispensary of the hospital, where Dr. Hughes showed him to Dr. Coppinger. He was admitted to the hospital, and sent to his ward, pending a laryngeal examination which was to have been made next day. However, that night the sister in charge of the ward was suddenly called, and on arriving at the bed she found life extinct. The next day the larynx was removed, and then the cause of death became evident. A large fibrous polypus, nearly spherical in shape, and three-quarters of an inch in diameter, was discovered, attached by a small pedicle to the anterior wall of the larynx, just below the left vocal cord. The growth itself had passed up between the cords, and lay upon them, completely occluding the rima. A beautiful preparation of the polypus *in situ* was made by Dr. Coppinger, from which the accompanying sketch was taken. [See Plate II.]

With such an instance before us, the propriety of removing such a growth from the larynx is evident—the manner in which such removal should be effected is not outside the range of dispute. Sometimes the growth may be removed easily, when it occupies a situation below the vocal cords, by the endo-laryngeal methods. But this is not always so. In the cases of growths in the larynx published by Morell Mackenzie, he mentions three cases in which the tumour occupied this position, and in which “evulsion was effected with great difficulty, and only after several months’ close attendance. In two of them, indeed, the success was incomplete—a small portion of the growth remaining, and the voice being still a little hoarse.” Out of the cases operated on by other practitioners, and collected by the same author, fifteen times was the growth below the cords. “In five of these thyrotomy was performed, and in two the less serious operation of tracheotomy was necessitated before the growth could be removed through the fauces. There is little reason to doubt that in many, if not all, of these cases, had

an opening been made in the crico-thyroid membrane, the treatment might have been reduced to a few days, or at most a few weeks, instead of lasting, as it did, several months, and that it is possible the results would have been altogether more satisfactory."

The details of the case recorded above abundantly show the correctness of the view expressed by this eminent laryngoscopist. The case affords another plea for substituting laryngotomy for the more dangerous and less successful operation of thyrotomy. Again, in most of these subglottic growths, laryngotomy promises a more rapid and more complete evulsion than can be done through the mouth, and by keeping the crico-thyroid wound patent for some time after the removal of the growth, recurrence is provided against. Had Dr. Burow attended to this detail in the case reported by him, he would have avoided the recurrence which marred his otherwise complete success.

ART. XIX.—*Notes on Cases of Fever treated in the Field Hospital, Camp Nicosia, Cyprus, in 1878.** By SURGEON-MAJOR JACKSON, C.B.

As most of the men who suffered from fever were Royal Engineers (as far as my charge extended), I may mention that the 31st company of sappers, with some telegraphers of 22nd and 34th companies, and the men of the Army Hospital Corps, whose cases are also included in my notes, left Southampton on 13th July, 1878, and disembarked at Larnaca on the 29th of same month. The strength of the 31st company was 3 officers and 155 men. The company marched from Chiflik Pasha on the evening of 5th August, and reached the Monastery camp on the morning of 7th August—the distance from camp to camp (30 miles) being divided into three marches; and from Surgeon Bennett's report it appears there were no cases of illness on the road.

The camping ground was close to a monastery, and about one mile from Nicosia, and the plateau extended for several miles, there being a gentle slope from the south-west to the north. This plain is a deep, rich alluvial soil, water being found at an average depth of forty feet. The land at this season is generally out of cultivation, except where crops of cotton and madder are

* Read before the Medical Society of the King and Queen's College of Physicians in Ireland, on Wednesday, May 7, 1879.

growing by means of artificial irrigation. Some scattered tops of olive trees and a few date palms give some slight variety to the brown and dreary expanse. The squalid and ruinous villages, built of sun-dried bricks, enliven the prospect but little. The plain is intersected by low, undulating limestone ridges; and to the west a chain of mountains, the highest point of which is Olympus or Troados, and to the north the rugged summits of the range on the north of the island, bound the view.

From the formation of the plain it is probable there is a subterranean watershed from the direction of the lofty range of mountains in the west. In addition to the supply of water from wells—many of these being worked by a Persian wheel driven by a horse or a mule—water is also distributed by means of aqueducts—this source being used both for drinking and irrigation.

The food supplied to the men was the ordinary ration, and of fair quality. Fruits—such as grapes, figs, and melons—were extremely cheap, and the men—as appears from the report of the medical officer—seem to have in some cases used them so freely as to have suffered from diarrhoea as a consequence. The aqueduct which passed close to the camp filled a large square tank and was used for bathing.

In his weekly report of 9th August, Surgeon Bennett, who was at the time in charge of the company, noted that of 10 who were on the sick list since arrival in camp, most were trivial; 3 mild cases of febricula; but in one case—the first of those annexed—the temperature on admission was 104·5°.

I reported myself at head-quarters, Nicosia, on the 6th August, and was directed to see an officer of the Control Department who was sick. His quarters were at the Bappho gate. I found him suffering from fever, with purging, vomiting, and well-marked exacerbations and remissions. The assistant Control officer was similarly attacked on the day following, and two non-commissioned officers and a Maltese servant had attacks of same nature. The surroundings of the place were enough to account for typho-malarial of the most virulent type. As you entered the gate a stream of urine from the prisoners' cell at the gateway trickled through the pavement; the buildings old and dilapidated; the latrine foetid beyond description; while outside the gate a sluggish black ooze from a currier's establishment close to the water festered and bubbled beneath the fierce Cyprus sun—in fact, the stink of the place was so palpable that, on one occasion, returning after dark

from a long ride, in company with Sir A. Home, and losing our way, I was at last able to determine our position by the aid of my nose. The Control officers suffered severely. The senior had not recovered his health or strength when I left Cyprus in November, and the other had several attacks of fever complicated by dysenteric diarrhoea, the temperature reaching 106° , and he had to be invalided to England.

In consequence of a great increase in the sick list at the Camp Chiflik Pasha, near Larnaca, the medical officer who had charge of the Royal Engineers was transferred, and it fell to my lot to look after the sappers, in addition to the charge of headquarter staff; and on the 16th August my weekly report was as follows:—“During the past week there have been numerous admissions with fever from the company of sappers encamped here. These cases appear under the heads of febricula and simple continued fever; however, from a careful examination of the symptoms and ranges of temperature, the disease appears to me, in many instances, to approach the type of bilious remittent.” Attention was also called to the insufficient protection afforded by bell-tents against the excessive ranges of temperature in the island. At that time the thermometer marked 67° in the early morning and 103° at midday, and subsequently, at the latter hour, it reached 116° .

In *The Times* of 20th August is a leading article commenting on a report from their correspondent in Cyprus, who had stated that the slight sun fevers were changing into agues; and, as it was inferred that the medical officers serving in the island were responsible for this unsatisfactory diagnosis, a suggestion was made to send out a physician or a medical committee to ascertain the nature of the disease. A month after a second article repeated the same suggestion.

The cases which I have the honour to submit appear to me chiefly interesting on account of the marked oscillations of temperature. I must apologise for the meagreness of the details; but my notes were chiefly taken in the gateway of the hospital—the only office available—and it is a difficult matter to keep up one's zeal when the temperature reaches 100° . I was much surprised to find such marked variations of temperature in the cases—the other symptoms, in many instances, not corresponding with the alternations. Wunderlich has given but little information relative to temperature of remittent fever. In the very high and low ranges I frequently took the degree both in the mouth and axilla, but

generally found the former the better. The instrument I used was one of Arnold's, and proved most satisfactory.

I will refer briefly to the cases noted in the temperature charts or diagrams.

CASE I.—Bugler Viney, when admitted, had a temperature of 104.5° , and, having been exposed to the sun all that morning (August 9th, 1878), he believed himself that his illness was the result of the exposure. There was no purging in this case till the 11th, the diarrhoea ceased on the 13th, and he was considered convalescent on the 16th. However, on the 22nd a relapse came on—his head being chiefly affected; vomiting and purging set in on the 27th, on which day he was transferred to Kyrenia, where he had another attack of fever on the 6th September. He was subsequently invalided to England.

CASE II.—Sapper Woodcock was under treatment in the Field Hospital, Nicosia, from August 11 to September 5, and the general symptoms during the time in many respects resembled typhus—the dull expression of face, black tongue, sordes on teeth, and low muttering delirium, closely simulated that type of fever. An Italian physician who saw the case—also one of the monks who had some experience in cases of the kind—pronounced it *tifo*. Purging was also present. He was transferred to Mathiadi on September 5th, and was subsequently invalided to England.

CASE III.—Sapper Corby. A well-marked case of bilious remittent; frequent attacks of greenish watery purging; a distinct remission on the 19th—the temperature, which reached 105.2° on evening of the 16th, and 104.8° on evening of the 17th, falling to 97.6° on morning of the 19th. Was sent to Kyrenia on the 27th August; had a relapse on the 30th; after this he mended slowly.

CASE IV.—Sapper Thompson. Also a case with marked variations of temperature. After the ninth day both pulse and temperature became subnormal—the former frequently below 60, and the latter on one occasion falling as low as 96.8° . In this case a large amount of albumen was found in urine on the 15th August, and on the 16th but a trace was detected. Skin and conjunctivæ were jaundiced; frequent epistaxis. He was transferred to Kyrenia on the 27th August. The medical officer there reported that Thompson had his spleen much enlarged, and suffered from leucocythemia. Was slowly improving on the 6th September.

CASE V.—Sapper C. Miller. Admitted August 12th with slight fever. Stated he passed blood from bowels. On the 14th blood and mucus passed, vomiting and purging; the stools on the 18th a bright green. Was transferred to Kyrenia on the 27th; had a relapse which lasted to September 4th; and was subsequently invalided to England.

CASE VI.—Sapper J. Barton. This case affords an example of

remittent approaching the intermittent form, with the marked evening exacerbations. The patient merely complained of headache. He was transferred to Mathiadi.

CASE VII.—Sapper J. Murphy. A severe case of fever. The evening of the day on which he was admitted his temperature was 106° , and on following morning had fallen to 100° ; on the 22nd fell to 98° , while on the 23rd the evening range touched 104° . Was transferred to Mathiadi on 31st August; temperature 97.4° . The report from the medical officer there stated "that Murphy was subject to attacks of ague, and as the man drinks more than he ought, he will not give himself a fair chance of throwing off the fever."

CASE VIII.—Sapper J. Gardiner. Very high fever when admitted; temperature 105.8° ; intense headache and pain in back; profuse sweat night after admission; large amount of phosphates in urine; no albumen; skin and eyes jaundiced. Suffered subsequently from debility and anæmia. Transferred to Kyrenia on 27th, where he suffered from an aguish attack.

CASE IX.—Sapper J. Cucksey. Fever not very high on admission. Nose bled twice; this relieved his headache. Skin and conjunctivæ soon after admission assumed a yellow hue, and the colour afterwards had a greenish tinge. The epistaxis became frequent, and increased the debility; and on 21st August blood was found to ooze from the mucous membrane at back of throat. Delirium and nervous jactitation set in on 23rd, and he died on 24th August. A *post mortem* examination four hours after death showed body well nourished; skin, lemon yellow; smell of body very offensive; no blood escaped when incisions were made. Lungs gray; nothing abnormal. Heart pale; red clot in left ventricle. Liver weighed 6 lbs.; at first the colour was a dark slate, after a few minutes' exposure to the air it became an olive green. Sections of the liver showed the same tint. Gall-bladder empty. The spleen weighed 1 lb. 5 ozs.; removed with great difficulty, being extremely friable; it was also a very dark green, and in consistence resembled black currant jelly.

CASE X.—Sapper Robert Drew. In this case, on first admission, although the fever was high, the bowels, costive at first, were not affected until four days after first accession of fever. The evening exacerbation was regular, so far as being higher than the morning during the first attack; but on re-admission an irregularity in this respect occurred.

CASE XI.—Sapper William Abernethy. It appeared that he had been ill and in hospital at Chiflik Pasha for about ten days before he arrived in this camp a few days ago. When brought to hospital was unconscious, and there was much difficulty in rousing him. He never rallied, and died with symptoms of cerebral effusion five days after admission. No *post mortem* examination.

CASE XII.—Sapper W. Baldwin. Fever commenced with headache

and purging—the stools watery—and green vomiting set in the day on which he was admitted. Area of hepatic dulness increased, and tender on pressure over liver. Vomiting and purging were prominent symptoms in this case. Was transferred to Mathiadi on 31st August.

CASE XIII.—Sapper W. Tucker. Reported sick on August 19th; fever high, and considerable cerebral disturbance; vomiting and purging persistent. On 22nd, delirium; tongue dry and glazed. In the evening was semi-comatose, and weakness extreme. He died on 26th August. *Post mortem* examination:—Body emaciated; thoracic viscera normal; liver, 4 lbs. 5 ozs., dark brown; gall-bladder contained about 2 ozs. bile; spleen, 12 ozs., colour and structure natural; kidneys, nothing abnormal.

As regards the treatment of the cases reported, quinine was used in doses from 5 grs. to 30 grs., and as a prophylactic in the smaller dose. It did not appear either to prevent an accession of fever or to cut it short when the attack commenced.

Remittent fever, in my experience, is but little amenable to treatment. However, one very distressing symptom—the intense headache—yielded in every case to blistering, applied in the severer cases to the scalp, and to the nape of neck when the pain was less intense.

Fortunately a supply of snow, stored near the summit of Olympus, was procurable for the sick, and proved most beneficial, either externally to the head when the headache and congestion were marked, or internally when sickness of stomach was a symptom. In many instances the convalescence was protracted, and the invalid had to be sent to England.

Dr. Sullivan, who has studied malaria in various parts of the world, states that fevers of the malarious type may arise from conditions of soil in which no marshes are to be found, and his description of the land of Cyprus, as rich, fertile, damp, and uncultivated, is perfectly correct. A short distance from the headquarter camp a shaft was sunk to get water, and 65 feet of alluvial soil was pierced without finding water or reaching the bottom of the alluvium. This was close to the dry channel of the river Pediceus.

ART. XX.—*Some General Observations regarding the Midwifery Forceps, and a few Special Remarks upon the Use of this Instrument in Modern Obstetric Practice.** By ROBERT FOSTER DILL, M.D., &c., Professor of Midwifery and Diseases of Women and Children, Queen's College, Belfast.

It is altogether unnecessary for me to attempt giving an account of the almost countless varieties of midwifery forceps which have been designed and furnished to the profession since the days of the Chamberlens, now over a period of more than two hundred years. I may be permitted, however, to say that the author of each variety believed there was a superiority and some great advantage in his own design over any other which had appeared before. For a long time, and until within the last forty years, the short forceps was, I may say, the only instrument in common use here as elsewhere. Now, however, it has almost altogether given place to the long forceps; and while I have always given the preference to Simpson's, I believe that Dr. Robert Barnes' long forceps is now received with more general favour. And this is even the case at present in the Dublin Obstetric School, where formerly almost every leading accoucheur had an instrument designed by himself and known by his name.

For so far we, in Belfast, have been satisfied to accept of, and make a choice from among those which have been furnished to us, and which have been recommended by other schools. Still Belfast has not been behind in the increasing but legitimate use of this most necessary and valuable instrument. For I remember well, when it was not much used here, that the late Dr. M'Clurcan, of Belfast, got into high repute in the town and neighbourhood, because of his very extensive and successful use of the midwifery forceps. Dr. M'Clurcan practised from an early part in the present century until near the middle of it, and from his day to the present time it has been growing in greater and still greater favour. One incident, and a remarkable one to us now, will strongly mark the difference between the past and the present practice. The late Dr. Samuel Thomson, of Belfast, one of the most eminent physicians of his day, in the north of Ireland, and who held a very large midwifery practice, told me, shortly before his death, that he had never used the forceps in his practice, which had then extended over the long period of forty-five years, and he died in the year

* Read before the Ulster Medical Society, March, 1879.

1850. This is a striking contrast to the junior practitioner of the present day, of whom I am safe in saying that he will not be in general practice one year without using the forceps more than a dozen times.

I presume I shall not, with my present audience, be considered as at all overstating the case if I assume that the midwifery forceps, its application and its uses, should be treated as amongst the most important and interesting questions for discussion in the whole range of obstetric practice, involving as it may do, the life or the death of both the mother and the child. Because, if the judicious and skilful use of this instrument be exercised, then safety to the mother and child must be the result. But if, on the contrary, the operation be proceeded with either untimely or by a clumsy and unskilful hand, then is it not more than likely that the consequences shall prove to be most disastrous? We know that in the beginning of and till far on in the present century, delay was practised to a dangerous extent, whilst latterly there has been more than the tendency abroad to an early, if not to the too free, use of the forceps, and especially in the Dublin School of Midwifery, where its introduction has been recently advocated by at least one leading accoucheur before even the os uteri has become fully dilated. I am not at present at all disposed to enter upon a defence of either the one extreme or the other, for in the one case injuries may be allowed to occur from delay and in being too tardy in entering upon the operation, whilst the other may unnecessarily lead to accidents by rashly acting too early. That we may avoid both of these dangerous errors—and which I shall take leave to designate as the Scylla and the Charybdis of obstetric surgery—I am rather inclined to advise here the adoption of a motto borrowed from Ovid's "Metamorphoses"—*Medio tutissimus ibis*.

In the table of statistics of delivery by the forceps, according to Churchill, we find that among British practitioners the forceps was used about once in 450 cases; in France, once in 140 cases; and in Germany, once in 107 cases. And now, when we consider the very frequent use of this instrument in modern practice, we must come to the conclusion, when the time arrives for making a return of the statistics of such cases again, it will be found that they far exceed the largest of the figures which we have quoted above. It may therefore be accepted as an admitted fact that this instrument is much more frequently used in the present day than it formerly was. And, notwithstanding all that has been stated, if

we except those cases in which the forceps has been used before the os uteri was fully dilated, I feel strong in the opinion that at no previous time within the history of obstetric surgery was it ever so successfully used as it is just now.

Amongst the most striking returns on this point are those contributed by Dr. Hamilton, of Falkirk, and which are worthy of some attention. Dr. Hamilton uses the forceps about once in every seventh or eighth case, and he has thus delivered 731 successive children without a single still-birth, which would form a remarkable page in obstetric history, were he able at the same time to record an equal number of successful results on the part of the mother; but, unfortunately, he makes no return, as far as I can discover, on this branch of his subject.

In further discussing this question I would beg to remind you that just as ergot is forbidden during the first stage of labour, so also should we forego the use of the forceps until at least we have reached the second stage of labour.

Dr. Leishman, in discussing this question, gives it as his opinion "that before the application of the forceps it is absolutely an essential condition there should be complete dilatation of the os uteri." And again he says, "An essential condition is, according to all authorities, that complete dilatation of the os should first exist before the introduction of the forceps."

Dr. Playfair has expressed the opinion, in his work on the "Science and Practice of Midwifery," "that the use of the instrument before the os uteri is completely expanded is a very serious matter." And Cazeaux, a very eminent French authority, while discussing this subject, tells us "that before we apply the forceps we should wait, in ordinary cases, till the membranes are ruptured and the os uteri fully dilated."

I could multiply such quotations almost *ad infinitum*, but I do not wish, by doing so, to tax your patience further upon this point, and the more so as I believe I have already proved to your entire satisfaction the correctness of my position—which is, that in ordinary cases of labour the forceps should not be used before the os uteri is fully dilated. But if more proof were needed, I have only, I hope, to appeal to your own experiences for a complete confirmation of the correctness of my statement.

I find, however, in the number of *The Dublin Journal of Medical Science* for last January, a "Report" presented to the Dublin Obstetrical Society by Dr. George Johnston, ex-Master of the

Rotunda Lying-in Hospital, in which he advocates "delivery by the forceps before the os uteri is fully dilated;" and he has satisfied himself that this practice is not only safe and justifiable, but also a great preservative of the lives of both mothers and children. Now, if we were able to satisfy ourselves, as he has done, then it would become a duty with us not only to read and discuss, but also to mark, learn, and practise, according to the rules laid down for our instruction in the "Report" to which reference has been made. But the discussion which follows upon that "Report" throws very important light, and affords some valuable information on the subject; and I think it but due to Dr. G. H. Kidd in particular to say here that in my opinion his statement is a very able, clear, and conclusive answer to Dr. Johnston's important "Report."

Dr. Kidd furnishes us with the statistics of what he is pleased to call "the old school," and he compares them with the information which he gleans from "the new mode of practice," as contained in the "Report," thereby eliciting the fact that the mortality which is found to have existed in the former is but 1·21 per cent., whilst in the latter there did occur a per-centage of 2·27, being about double the number of deaths in Dr. Johnston's practice when compared with the practice of the masters who had gone before him in the Rotunda Lying-in Hospital. But when we come to that part of the "Report" which relates more especially to those cases in which the forceps had been used before the os uteri was fully dilated, we find that the result is still worse; for of the 164 cases delivered with the forceps before the os uteri was dilated, and in which cases there cannot be discovered anything beyond the fact that they were ordinary cases of tedious labour, six deaths are recorded, being at the rate of 3·6 per cent., whilst in tables of tedious labour compiled by Churchill the mother's mortality is put down at nil.

I might accumulate comparisons of this kind further, but I think I have demonstrated the fact that delivery with the forceps before the os uteri is fully dilated is unsafe, and, consequently, should be considered as unsound in practice.

Under such circumstances as these, and with the facts and the figures before us, is it necessary that I should formally ask the question are we to adopt the teaching, or, as it has been termed, the "new mode of practice," as recommended in Dr. Johnston's "Report;" or shall we adhere to, and pursue, the more moderate means of delivery? For my own part I have no hesitation in

stating that I see no reason why I should give up the latter course, as it has been proved to be the safe one; whilst the former I consider not only a dangerous course, but nothing short of an entrance upon a downward if not fatal career—a career to which we might not be far beyond the mark by applying a not inappropriate expression from Virgil's "*Æneid*"—*Facilis descensus Averni*.

When I first heard of this "new mode of practice" I was almost tempted to apply to it, in a modified form, the Russian expression, which was used in regard to the light cavalry charge of "the Six Hundred"—that it was *une charge magnifique, mais une charge de fous*. So of the "new practice;" it was *une opération magnifique, mais une opération de fous*.

Although I have spoken thus strongly, perhaps too severely, it is only in regard to the use that has been made of the forceps before the os was dilated. At the same time I might be misunderstood if I stopped here without saying that there are certain cases to which the foregoing statements do not apply—such as in the case of a somewhat narrowed pelvis, and in which the os uteri does not become fully dilated because of the forces not being brought to bear upon, or directed towards, its axis, so that in this case, and from a few other causes that demand its use, the period of labour for applying the forceps must necessarily vary.

And now, Mr. President, I have only to say in conclusion (after apologising for trespassing upon your patience with such an imperfect paper) that *time* would appear in this, as we know it has become in other matters, the absorbing interest of the age. And I think I hear it asked why should we be behind in the race? From my own experience I am much inclined to counsel some prudence and a little more delay. For although we may, by assisting to bring about a hasty termination to a case, win the applause of the moment, we may rest assured that the calm and thoughtful physician, and the skilled and patient accoucheur, will in the long run have a more just and a more abiding reward.

ART. XXI.—*Villous Disease (Papillary Fibroma) of the Bladder, and its Surgical Treatment*. By ROBERT S. HUDSON, M.D., M.Ch., Q.U.I.; L.R.C.S.I.; Redruth, Cornwall.

THE case on which this paper is based may be considered worth recording on several grounds—its long duration, probable cure by injections, recurrence of disease, and *post mortem* appearance, which

plainly showed that the growths might readily have been removed by surgical interference.

Villous disease of the bladder is a rare affection. Dr. Goodhart, of Guys, has told me that it is not infrequently seen in the *post mortem* theatre, but the experience of most surgeons points to its being rarely diagnosed during life.* Although called by the term "villous cancer," there is nothing of a malignant character attaching to the growths; they are extremely vascular, and consist of mere loops of blood-vessels covered by epithelium, which in no case grows *within* the delicate framework of connective tissue.

I propose to abridge the voluminous notes which were taken during the patient's prolonged illness, by arranging the prominent points under certain heads.

CASE.—Family History.—Three brothers and his father died of obscure vesical or prostatic disease, said to be "cancer" in the case of two. I wrote to the medical attendant of one, but he could not give me any definite information.

Personal History.—Always enjoyed good health until 1867, when he observed his urine to be discoloured, and he required to get out of bed at night to relieve his bladder. He was then fifty-four years of age, and consulted my predecessor, the late Thomas Michell, M.D., Lond. During Dr. Michell's attendance there was complete obstruction to the flow of urine. A consultation was held; the bladder was found to be distended with blood clot, which was broken down with a catheter, and removed by the help of Clover's syringe. So far as I could make out, after the lapse of time, cystitis supervened, and blood used to come away in the urine in large quantity. Dr. Michell believed the bladder to be ulcerated, and he continued to inject a solution of nitrate of silver at intervals for several weeks. The patient remembered the particulars very distinctly, as the pain of injection was so great as to require his being placed under the influence of chloroform on each occasion. At any rate he was completely cured, returned to his occupation as a respectable tradesman, and continued in good health for nearly eight years. He looked upon himself as a marvellous recovery; and previous to his late illness, which I interpret as a recurrence of the disease, I was inclined to believe he exaggerated (as old soldiers are reputed to do) his former danger.

In January, 1875, he first consulted me, and said that his former attacks began in the same way. At intervals of three or four days, blood would appear in the urine, and for many months I could not make up my mind as to the cause.

* Mr. Birkett (Med. Chir. Trans., Vol. XLI.) tabulates ten cases, being all on record up to the date of his paper, March, 1858, and including Warner's, A.D. 1737.

Diagnosis.—His age, the family history, abundant hæmorrhage, and emaciation pointed to “malignant disease,” and microscopic examination of the sediment, with its suspicious-looking cells, favoured that view; but the long duration, absence of lymphatic deposit, or enlargement of neighbouring organs, and finally detection of pieces of villous structure in the urine, confirmed me in a correct diagnosis. For a long time I suspected “stone;” but the patient, having missed the omnibus one evening at Falmouth, was compelled to walk home nine miles, yet neither that day nor for three days afterwards did any appreciable amount of blood appear, which in the case of “stone” must surely have been aggravated. He had such a dread of catheterism that I could not get him to consent to the introduction of a sound, except when under the influence of chloroform; and as he had dilated heart, with well-marked *arcus senilis*, I did not consider myself justified in using an anæsthetic, unless some more serious symptoms should be presented. Pain and frequency of micturition were his most prominent causes of complaint—the pain preceded and followed micturition—both were relieved by morphia suppositories; but as the disease advanced, and the ureters became enlarged, the pain assumed a deeper, heavier character, extended to the loins, from the hypogastric to the lumbar regions of the abdomen and the back. Clots of blood were rarely seen in the urine; at no period when floated out did they present the worm-like coils indicative of coagulation in the ureter, but were irregular and flattened, as if formed in the bladder. Blood seemed to be present without any known exciting cause. Sometimes for days the urine would be quite free from any tinge, and one was hoping that the astringent medicines were doing good; and on another occasion, with precisely the same treatment, the urine would be so charged with it as to resemble raw meat juice more than anything else.

Careful and repeated examination on Sir H. Thompson’s method—viz., by having the sample of urine divided into two portions, one representing the flushing of the urethra and the washing out of the canal with its epithelial *debris*, and the second portion the fluid as it exists in the bladder—satisfied me that the hæmorrhage was vesical in its origin, and neither from the kidney nor ureter; but until the passage of pieces of villous tumour, which settled the diagnosis, I must confess the microscope was more a hindrance than a help—and no wonder, when we remember the various forms of epithelium which may be developed on the ulcerating surface of a new growth on a mucous membrane. “Prostatic disease” was also suspected, but this was negatived by rectal examination, which showed that the prostate was not enlarged, and that behind it the bladder was contracted with thickened walls, but containing within it some soft, yielding contents, of a doughy character. The emaciation was not at any time that of the cancerous cachexia. For more than

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Fig 1

Fig 2

x 40 diam

A Fig 2 x 310 diam

eighteen months his appetite remained good, even when the hæmorrhage was most constant, and, although anæmic, no wasting occurred. When the appetite failed, and sources of bodily heat lessened, he confined himself mainly to his room, became still more anæmic, and, with the aid of morphia, passed a tolerable existence.

Termination.—For three months previous to death the hæmorrhage had entirely ceased, portions of phosphatic deposit were occasionally voided, appetite was fair, a suppository only required every third night, and, to all appearance, he was gradually improving. In April, 1878, on attempting to get into bed after relieving the bladder, he fainted and fell. His son, who had only been out of the room two minutes, heard the fall, rushed up-stairs, sent for me, and as I happened to be passing on the streets, a few yards from the house at the time, I quickly arrived; the pulse had ceased to beat, and all was over in a few seconds from fatal syncope, probably connected with the diseased heart.

Autopsy.—Owing to the family history, an examination was readily granted. I have had a sketch of the bladder, with its tumours, lithographed, half original size. It will be seen that there are eight tumours, each connected by a narrow pedicle which might be ligatured, avulsed, or treated with the ecraseur. The large tumour at the fundus has been reflected, so as to show more distinctly the pedicle from which it springs. All were coated with a thick layer of phosphatic deposit like brownish mortar. When this layer was removed, and a portion of the tumour floated out in water, its true character was seen—delicate lace-like growths springing from a firm, fleshy pedicle. To the right will be seen the enlarged projecting bundles of an hypertrophied bladder, not unlike the *columnæ carneæ* of the ventricles of the heart. The openings of the ureters may be seen, but the ureters themselves, owing to the obstructed flow of urine, were dilated to the size of an œsophagus.

The innocent character of the growths may be learned from the microscopic drawings. Fig. 2 represents a portion of a tumour under an inch object glass; Fig. 3, a portion of the same under a quarter of an inch object glass. The thin walled capillary vessels, of irregular diameter, may be seen coursing through the growth—the whole surface being covered with a columnar or polygonal epithelium. This epithelium is, however, *on* the surface, *homologous*, not *within* the subjacent connective tissue, *heterologous*, which is characteristic of epitheliomatous growths.

“Villous” is the term by which these growths are best known, but it is, perhaps, better to adopt the more recent name—papillary fibroma—which at once gives the clue to their structure and clinical character.

Treatment.—The consensus of opinion which pervades all English authorities within my reach as to the merely palliative treatment of this affection is remarkable, when we read that these growths have been frequently removed in the female with success. I cannot but think that it arises from the term “villous cancer,” a misnomer which is still retained in some of the text-books. My sole object in publishing this case is to show the feasibility of operative interference when the diagnosis is certain and the general symptoms favourable.

Bryant says : “ There is no cure for this affection—the surgeon can only relieve symptoms.”^a Sir H. Thompson, in his Lectures,^b and in the article in Holmes’ Surgery, confines himself to instructing us how to combat the prominent symptoms of pain, hæmorrhage, and frequent micturition. Coulson says: “ By way of treatment, all we can do is to allay pain and irritation by the use of sedatives,”^c but, further on, after quoting in detail the case of Crosse, of Norwich, he mentions that Mr. Warner removed successfully, by ligature, a tumour growing from inside the bladder. Fleming,^d Druitt, and Spence do not refer to it. Erichsen, it is true, mentions Warner’s and Civiale’s cases, but the whole subject is dismissed in a few lines. As I had only these authorities to consult, I treated my patient with various astringents for the hæmorrhage, and administered sedatives for the pain and frequent micturition. I do not think that any of the astringents were of real service; sometimes I fancied they were, but each failed at times; and, looking at the tumours and their delicate processes—liable to be torn or squeezed at any time by the contraction of an hypertrophied bladder—I cannot see how any medicine administered by the mouth can produce much local astringent effect on these growths in any hollow viscus such as the bladder. The medicines employed for the hæmorrhage were—Gallic and sulphuric acids, acetate of lead, liquor of the pernitrate of iron, turpentine, ergot (liquid extract of the U. S. Pharm.), and tincture of hamamelis. Hypodermic injections of Morson’s ergotine were also tried. For the next indication—the relief of pain and spasm—nothing suited so well as half-grain morphia suppositories. Ten minutes after the introduction of one, all pain and spasm would disappear, and

^a Practice of Surgery. 1st Ed. P. 505.

^b Diseases of the Urinary Organs. 4th Ed.

^c Diseases of the Bladder. 4th Ed. P. 209.

^d Clinical Records on Diseases of the Gen. Urin. Organs.

when used at bed-time, he did not require to empty the bladder for several hours, thus ensuring sleep.

Over and over again I thought of the possibility of operative interference by removing the growths after incising the bladder through the perinæum; but it was only last August, when attending the Bath meeting of the British Medical Association, that I saw in the daily journal that Professor Humphry, of Cambridge, had successfully removed a growth from the male bladder.^a Later on I read in *The Lancet* the most interesting communication of Dr. William Alexander, of Liverpool, detailing a case of villous growths in the female bladder, which were removed partly by polypus snare and partly by scraping after dilatation of the urethra. Although she had been suffering from so-called chronic cystitis for nine years, the relief was so great that she was able to transfer herself from the wards of a workhouse to be cook in a large family, and earn her own living.^b Later on still, I came across Gross's "Diseases of the Urinary Organs," and there I found not only the fullest account of the disease in question, but also the confident statement that, "for the relief of papillary and polypoid fibromas, surgical interference is imperatively demanded, since without it a fatal issue is almost the inevitable result. In male subjects the only rational mode of attacking these growths is by opening the bladder and removing them, in accordance with the extent of their attachments, by enucleation, avulsion, scraping, ecraseur, or ligation. In females, on the other hand, cystotomy is uncalled for, since, on account of the greater shortness and dilatability of the urethra and the absence of the prostate, access to the tumour is rendered easy and attended with less risk."

Dr. Gross presents the statistics of sixteen operations in a tabular form. The cases of Warner, Crosse, Birkett, and Hicks, are included. To these should be added those of Humphry, Alexander, and Lawson Tait,^c and the summary will be as follows:—Fourteen females and five males. Of the adult females, all recovered except one—death in this case being from perforation of the bladder in attempts to tear off a sarcomatous tumour by forceps. Of the five males three recovered. In children, according to Dr. Gross, the operation holds out little prospect of relief, owing to the number of tumours rendering them inaccessible except by suprapubic incision.

^a Brit. Med. Journal. Vol. II. 1878. P. 368.

^b Lancet. Vol. II. 1878. P. 209.

^c Diseases of Women. P. 81.

In females the removal of these growths must be a comparatively easy operation, as, after rapid dilatation of the urethra, when the patient is anæsthetised, all parts of the bladder can be readily reached. I do not think that practitioners are generally aware of the curative results which very frequently follow rapid dilatation of the urethra in various diseases of the female bladder. It is easily done by a few large size gum elastic catheters or small vulcanite rectal bougies, or the dilator of Messrs. Weiss, or the hard rubber specula of Professor Simon, of Heidelberg, who has scraped off papillary fibromas in three cases, with recovery in each, although in one the bladder was nearly two-thirds filled with these growths. Mr. Teale's essay "*On the Treatment of Vesical Irritability and Incontinence of Urine in the Female by Dilatation of the Neck of the Bladder,*"^a is so clear as to convince the most sceptical, and lead him to aim at similarly satisfactory results, when treating a most distressing class of complaints.

Of the five males, one was Crosse's, of Norwich. A child two years of age had suffered for six months, and although after repeated examinations no stone was satisfactorily felt, Mr. Crosse thought one might be encysted at the termination of the left ureter, and as the child was evidently sinking, he determined to attempt an operation for his relief. After cutting down to the staff, and opening the membranous portion of the urethra, soft tumours protruded from the wound; in appearance they resembled several coils of small intestine. A large quantity was removed, but much of the diseased structure was left behind, and the little patient died after forty-four hours' incessant suffering.

The second case was a man, aged forty-nine years, operated on by Gersuny, quoted by Dr. Gross from Langenbeck's Archives, 1872; but in it the growth was not reached, and the patient died.

The third was one which coexisted with stone, and was avulsed by Desault. Recovery followed.

The fourth was Billroth's celebrated case.^b Patient discharged from hospital on thirty-second day, wearing a truss to prevent hernial protrusion from suprapubic cicatrix. Billroth first verified his diagnosis by cystotomy in the perinæum; then, in order to have plenty of room, he divided the recti muscles at their insertions, and incised the bladder transversely. After removal of the growths

^a *Lancet.* Vol. I. 1876. P. 84.

^b *Brit. Med. Jour.* Vol. II. 1875. P. 498.

a drainage-tube was allowed to pass through the organ, and hang out of the lower opening.

The fifth case was the one of Prof. Humphry, already referred to. A young man in Addenbrooke's Hospital had suffered from frequent micturition, agonising pain, straining, &c., with discharge of bloody urine. An incision, as for lithotomy, was made into the bladder. A pedunculated ragged mass of the size of an orange was torn through with the finger, and its root scraped with the finger-nail. He quite recovered, and six months afterwards was well.

Even if it were found, after performing cystotomy, that the tumours could not be removed with safety, the operation would, in most cases, benefit the patient, as ready exit would be given to mucus or other deposit, and all the agonising pain and strain which accompany an hypertrophied bladder would thereby be lessened. In America cystotomy seems to be a recognised operation for various chronic diseases affecting the male bladder, just as rapid dilatation of the urethra is considered curative in certain chronic diseases of the same organ in females.

To conclude—

1. Villous disease of the bladder is not so rare as is generally supposed—many so-called cases of chronic cystitis being probably due to it.

2. Its diagnosis is most difficult, and can only be arrived at after long observation, and by a process of exclusion.

3. Urinary deposits containing so-called cancer cells are very misleading, but the microscope is most valuable in detecting small portions of genuine villous growth.

4. There should be no difficulty in detecting the growths in the female, as the whole internal surface of the female bladder can be readily explored with the finger after rapid dilatation of the urethra, when under the influence of an anæsthetic.

5. Astringent injections are likely to be of use in the early stages, and before the growths have become pedunculated.

6. The surgeon, while unsparing in the use of sedatives to relieve pain and spasm, should bear in mind the possibility of permanent cure by removal of the growth.

7. Statistics show that the operation is neither difficult nor dangerous in the female; and there are good grounds for believing that when preceded by cystotomy in the adult male it will prove justifiable and satisfactory.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Atlas of Histology. By E. KLEIN, M.D., F.R.S., and E. NOBLE SMITH, L.R.C.P. & M.R.C.S. Parts I., II., and III. London: Smith, Elder, & Co. 1879. 4to.

THE impetus which the study of microscopic anatomy has received in this country within the last few years is to a very large extent due to the labours of Dr. Klein. The anatomical part of the "Handbook for the Physiological Laboratory," with its numerous and beautiful drawings; the researches on the anatomy of the lymphatic system; the various papers on the terminations of the nerves, and other subjects; and, above all, the personal instruction which so many students have gratefully to acknowledge having received from Dr. Klein himself, have all tended to produce the present condition of histological knowledge, which contrasts so favourably with that which prevailed a short time ago.

In the parts before us we have the first instalment of a work which promises, as far as illustration goes, to be the most magnificent systematic work on animal histology which exists in any language.

The three parts contain fifty-four pages of letterpress and eleven plates, comprising numerous figures, mostly coloured. The drawings have been made by Mr. E. Noble Smith from preparations by Dr. Klein. Both as regards the faithfulness with which the objects are delineated and the artistic beauty of the execution we can speak of them only in terms of the highest praise. The text is very brief; all controversy being avoided. The authors tell us that the object of their work is to teach not so much the history of histology as histology itself; and, in accordance with this aim, they describe what they believe themselves to be the structure of each part, and do not record the various conflicting opinions of other writers. The work is divided into chapters, which treat of the tissues and organs in the following order:—Blood, epithelium and endothelium, connective tissues, muscular tissue, the nervous, vascular, and

lymphatic systems; then follows a short chapter on cells in general, after which the compound tissues are considered *seriatim*; the alimentary canal and its glands; the respiratory organs; the urinary and genital organs; the skin and special-sense organs. The concluding chapter treats of organs the nature of which is not sufficiently well known—as the suprarenal capsules, the thyroid and coccygeal gland. Part III. carries us to the end of the chapter on Cartilage.

We shall indicate briefly a few of the more novel points brought forward.

A network of fine fibres occupying the nucleus has recently been described by many writers in cells of different kinds. Dr. Klein has shown that this fibrillar network is met with in all nuclei, and that it is continuous through the nuclear membrane with a network which traverses the cell-body. The meshes of this network are occupied by a hyaline substance. The granular appearance which the protoplasm commonly presents is either due to imperfect preparation or is the expression of the fibres seen in optic section and appearing as points. The intranuclear and intracellular networks are described in red and white blood-corpuscles, epithelial and endothelial cells, and connective-tissue and cartilage corpuscles. The cilia of the ciliated epithelia are shown to be continuous with the intracellular network, to whose contraction their movements are probably due. Also, the spines or prickles of the cells of the rete Malpighi and of the deeper cells of the stratified epithelia are projections of the intracellular network, by means of which neighbouring cells become continuous one with another.

The spontaneous movement of the nucleus of the white corpuscles of the blood of the frog and newt, observed by Stricker, is confirmed, and is attributed to the contractility of the intranuclear network.

The crinate or horse-chestnut shape which mammalian red blood-corpuscles so often present is supposed to be due to a loss of carbonic acid, since a current of this gas restores the corpuscles to their original discoid form.

The nucleolus is an organism to which very little importance is attributed. It is considered to be either a remnant of the primary substance out of which the intranuclear network has developed or a part of this network shrunken in consequence of imperfect methods of examination.

The formation of goblet cells is due to a conversion of the

hyaline substance contained in the meshes of the intracellular network into mucus, which is hygroscopic, and, consequently, swells up. By this swelling the nucleus, with its small portion of surrounding protoplasm, is pushed to the deeper part of the cell, and the free surface of the cell-membrane finally yields, giving exit to the mucus. With all this, the intracellular network persists, but with widened meshes; and, in his recent paper in *The Microscopical Journal*, Dr. Klein has adduced strong reason to prove that the transformation into goblet cells is not final, but is merely a stage in the process of secretion, on the completion of which the cells may resume their original appearance.

The extension of the processes of connective-tissue cells between the cells of epithelium is maintained, and the basement membrane of many mucous surfaces is stated to be composed of endothelial plates, as originally described by Debove.

“In the case of the cornea, serous membranes, and other membranous expansions of connective tissue—*e.g.*, the connective-tissue lamellæ that can be obtained from the subcutaneous or submucous tissue—it can be shown that each connective-tissue corpuscle is composed of two distinct substances: (*a*) a hyaline plate—*ground plate*—which contains the oval nucleus, the substance of which is a dense network (intranuclear network), and (*b*) a second substance—a network of minute fibrils (intracellular network), arranged always more copiously at one side of the nucleus than at the other, and extending from here beyond the limits of the ground plate, as finer or thicker branched or unbranched processes. This second substance is in connexion with the intranuclear network. The ground plate extends only on the more membranous processes.”

In those animals whose omentum, at first continuous, subsequently becomes fenestrated, this change is not due—as maintained by Ranvier—to the perforation of the membrane by wandering leucocytes, but “is produced by cavities appearing between the connective-tissue bundles, which cavities open through the interstitial cement-substance of the surface-endothelium. The connective-tissue corpuscles situated previously between the connective-tissue bundles are then used to cover the lateral surfaces that have now become free; so that a direct transition of connective-tissue corpuscles into endothelial cells of the surface is hereby established.”

The development of fibrous connective-tissue from the embryonal connective-tissue corpuscles is described as taking place according to two methods—either by a transformation of the cell substance

into fibres, the nucleus disappearing, or by the production of a homogeneous intercellular substance in which the fibres are subsequently formed. The direct mode of formation of the fibres from cells is observed in tendon, skin, intermuscular tissue, nerve trunks, loose cellular tissue, &c. In the umbilical cord and serous membranes there is, besides this, also the other—viz., indirect mode of formation. Elastic fibres are formed by direct transformation of cells and their processes, with disappearance of the nuclei.

The vascular supply of adipose tissue is supposed to give it characters which place it in a group with glandular tissue.

“In some places—as in the subcutaneous and submucous tissue, intermuscular and other loose connective tissue—the fat-cell tissue is derived from ordinary fibrous connective tissue, in which the connective-tissue cells, having increased in number, become changed into fat-cells (Flemming); increasing in size, this tissue becomes richly vascularised. In other places—*e.g.*, serous membranes—the great mass of the fat-tissue is derived from a peculiarly-changed connective tissue (Klein); in many places (especially in connexion with the large vessels) there appear in the connective-tissue matrix of the serous membrane, patches, nodules or cords, which are made up of multiplying connective-tissue cells. As the number of the cells increases, the matrix in which they are embedded becomes transformed into a reticulum; lymph-corpuscles appear amongst them, and after the tissue has become richly vascularised by an afferent artery, efferent vein (or veins), and a rich network of capillaries, it resembles, in morphological respects, lymphatic tissue. The serous membranes of many mammals and man possess, in the young and adult state, a considerable amount of this species of lymphatic tissue. In some animals it is found to have a greater development than in others; in some places it is arranged more in the shape of cords or patches, in others more as nodular masses along the larger branches of vessels which supply them with their vascular system. The small—*i.e.*, the youngest, cord, patch or nodule, does not possess any vascular system, but this soon appears as that organ enlarges; first it is represented by a simple capillary loop, derived either from a neighbouring patch or nodule, or directly from a larger vascular branch; then numbers of new capillaries are formed, either directly in connexion with the existing capillary vessel, or independently of this, from connective-tissue cells of the patch or nodule, in a manner which will be described minutely in the chapter on Blood-vessels; one branch of the original capillary loop is changed into an artery, the other into a vein, and we have then a patch or nodule of lymphatic tissue possessed of its own system of blood-vessels. By elongating and fusing, they (patches or nodules) form a longer or shorter cord. As has been mentioned in a previous chapter, generally one or,

occasionally, both surfaces of these lymphatic structures are covered, not with the ordinary flat large endothelial cells, but with germinating cells."

These lymphatic masses in some cases—as in carnivora, and the rat and mouse—readily undergo transformation into fat tissue; in others—as in man, rabbit, and guinea-pig—they are persistent and function as lymphatic glands, and only under peculiarly favourable circumstances become changed into adipose tissue.

The authors hold, like many other modern histologists, that the intercellular substance of hyaline cartilage is traversed by fine canals, which effect a communication between neighbouring lacunæ and form the lymph canalicular system of the cartilage, communicating with the lymphatics of the perichondrium.

From these extracts it will be seen that the work is one of great originality. Its chief value, however, lies in the drawings; and this value will be permanent, whatever may be the ultimate fate of the views put forward by the authors in explanation of the appearances which they figure.

Defective Medical Education the Chief Obstacle to a proper appreciation of our Profession by the Public, and what our Alma Mater is doing to remove it. By W. A. B. NORCOM, M.D. Philadelphia: Collins. 1878.

THE above is the title of an address delivered by Dr. Norcom to the meeting of the alumni of the Medical Department of the University of Pennsylvania. The author takes as the text of his address a quotation from a speech by a *confrère* delivered at New York some years previously:—"With all our improvements and advances, we are painfully conscious that the relations existing between the medical profession and society, especially the educated classes, are far from satisfactory, or complimentary to us." We have been noticing a somewhat similar complaint in this country. We are very glad indeed to find that Dr. Norcom agrees with the best-informed members of the profession on this side of the Atlantic, that when we find our relations with the "educated classes" "far from satisfactory, or complimentary to us," that it is our own fault, and that he thinks "it is the custom to attach too much blame to the public, and too little to ourselves." These are sound, honest, out-spoken sentiments, which must commend themselves to every well-educated member of our profession. Dr. Norcom contrasts the education demanded by the American medical

colleges with that required in "Great Britain and on the Continent of Europe." He declares that the American colleges compare most unfavourably with those on this side of the Atlantic, and states that, "with the exception of about half a dozen, the longest term of lectures in any of them is scarcely five months, and attendance in only two terms is required for graduation." "This is all the time *required* to be spent at these schools, but the old stereotyped phrase appears in their 'announcements' that the applicant for a diploma 'must have studied medicine three years under a respectable practitioner—the time spent at lectures being included.'" It is pleasant to find our efforts to obtain medical reform seconded by our American cousins; but what must we think of their system when we find ours held up as an example to be copied! We congratulate the Society of the Alumni of the Medical Department of the University of Pennsylvania on the position they have taken up as pioneers of medical reform in the United States of America, as their ancestors were the pioneers of civilisation in the same great country.

The Fight with Infection: what to do when Scarlet Fever, Diphtheria, Typhoid, &c., enter our Homes. By WILLIAM STEPHENSON, M.D., F.R.C.S.E.; Regius Professor of Midwifery and the Diseases of Women and Children in the University of Aberdeen. Aberdeen: D. Wyllie & Son. Edinburgh: Maclachlan & Stewart. 1879.

THIS pamphlet professes to be a sort of popular guide to the prevention of the spread of contagion. We are always inclined to be lenient in our criticisms of such works, and indeed the pamphlet before us is in many respects a good attempt at the attainment of the object its author has in view. We must, however, protest against the mere opinions of an author being introduced into such a work as this. He seems to make light of the carrying of infection by third persons. Thus he says, writing of scarlatina and measles:—"If a person does not actually enter the sick room, and if further he passes some time after in the open air, there is no likelihood of his carrying home any infection by visiting an infected house." There is a great deal of this *making little* of the danger of infection, which is extremely injudicious in a work of this sort. Here is another dangerous statement to put before the public:—"We may leave for discussion by scientific men the

question of spontaneous origin of diseases. It is still a very doubtful question, and year by year is becoming more so." The question should, no doubt, be left to scientific men, but is the question becoming "more" doubtful? We cannot think so. The question is by no means settled or nearly settled, but certainly it is not becoming *more* doubtful. We are told that carbolic acid of a strength of one in forty is sufficient for a disinfecting wash, but are informed that "carbolic acid soap is too weak to exert any influence on the contagion." We have often found carbolic acid soap, such as it is our custom to use, so strong that the smarting it produces is a serious objection to its use. We are afraid Dr. Stephenson's experience of carbolic acid soap is limited to one or two of the weaker kinds.

Here is a curious paragraph:—"Amongst the poor who have only one or two rooms, and cannot leave their house, reliance must be placed on fumigation, and afterwards good cleansing and free ventilation. The rich can send away their bedding to be purified and cleansed, but the poor must rely on their own efforts." How can people "who have only one or two rooms, and cannot leave their houses," rely on fumigation, which can only be efficiently performed when they do "leave their houses?" Why "*must*" the poor "rely on their own efforts?" We should think the poor should not be expected to rely on their own efforts, and indeed in all districts managed by intelligent sanitary authorities they are *not expected* to "rely on their own efforts." We are afraid sanitation in Aberdeen must be in an even more backward condition than it is in "dear dirty Dublin." The author dogmatically separates chickenpox from smallpox as a separate disease, although he must well know that such a specific separation is by no means universally recognised. We regret to find rules for diagnosis laid down in a popular work of this description. Thus we are told of typhus fever:—"The symptoms of this disease need only be referred to in general terms to show its distinction from typhoid." Will a reference in "*general terms*" show this distinction? We think not, specially when we find it stated of typhoid or enteric fever:—"The symptoms of this disease are very variable." We should think they are "*very* variable," and so often resemble typhus that the diagnosis is difficult to the experienced physician, even though Dr. Stephenson considers the latter can be distinguished in general terms.

Dr. Stephenson's zeal for imparting knowledge has far outrun

his discretion, and we trust if he should ever venture to write a popular treatise concerning his own department of medical science, that he will weigh his words more carefully before he prints them.

Domestic Medicine and Hygiene. By WILLIAM J. RUSSELL, M.B.
Second Edition. London: W. H. Everett. Pp. 440.

THE preface of this work states that its design is to give a "short but accurate account of the principal diseases, their cause, nature, treatment, preventive and curative, in plain language, as free from technical terms as possible, so as to form a guide for those compelled to rely on their own resources." The book is thus ostensibly intended for non-medical readers, and the author believes that in "mild cases of many common diseases it is quite possible for any person of average intelligence to treat them satisfactorily by aid of written instructions;" but, at the same time, when diseases of such gravity as typhus and enteric fever, smallpox and apoplexy, not to speak of various acute affections of the eye, are described, with their appropriate treatment and medicines, it is hard to resist the conclusion that the writer also aimed at supplying a handy little cram-book for students or routine practitioners.

RECENT HOSPITAL REPORTS.

1. *First Annual Report of the Home Hospitals Association (for Paying Patients)*, 1877-78. London: Harrison & Sons, St. Martin's-lane. 1879.
2. *Twentieth Report of the Board of Superintendence of the Dublin Hospitals.* Dublin: Alexander Thom. 1879.

THE Home Hospitals Association seems fairly on its legs, and we wish the promoters of the movement all the success they deserve. It might be thought that an association with influential supporters, and which easily obtained the capital required to start with, would have little difficulty in finding a suitable habitation in London. This, however, was not so, for the promoters seem to have met with obstructions at every point where they sought house accommodation. They have at last vanquished the obstructionist of Manchester-square, and established themselves in Berkeley House as the first Home Hospital, in the middle of the West End.

The Association has determined to make no profit by way of

dividend to its proprietors out of the Home Hospitals. This seems a doubtful policy, for we cannot see why philanthropic people should be expected to sink their money for the benefit of well-to-do people. It is probable that the promoters of the undertaking thought it best to begin experimentally, and without the chance of an imputation of commercially speculating on the sick. We believe the whole of the success achieved by the Home Hospitals Association may be attributed to the energy and organising ability of Mr. Henry C. Burdett, who seems to have devoted his life to the organisation of hospitals, from the cottage to the palace.

The twentieth report of the Board of Superintendence of the Dublin Hospitals presents the same dreary aspect as its nineteen predecessors. On the first page we find the Board states that while "making these inspections, and on other occasions, we have conferred with the medical officers respecting the general administration of each institution." This will be news to most of the medical officers of the Dublin Hospitals. On what "other occasions" except at the annual inspections did the Board *ever* confer with the medical officers of any institution? The conferences with the medical officers about "general administration" have been, so far as we can ascertain, of the most shadowy description possible, and on "the other occasions"—probably shadows of shades. The remarks upon the management and arrangements of the hospitals inspected show a want of elementary knowledge on the part of the Board which is truly lamentable. The report bears date the 7th of October, 1878; it was not printed until 1879, and was not in circulation until March, 1879. Our readers—if not already acquainted with the tardy proceedings of the Board—will be surprised to hear that the report with the foregoing dates professes to deal with the year ended March 31st, 1878. It took nearly a year to put together this report of 25 octavo pages. We notice that these reports are *habitually* a year late. We observe an important omission from the tables contained in the report—namely, a table of the attendances of the members of the Board at the inspections of the hospitals. Some of our hospital friends tell us that very few—frequently the chairman and one other member—constitute the Board of inspection. In looking at the signatories of the report we miss the familiar names of William Stokes and Dominic J. Corrigan, Bart.,

who for many years represented the *physicians* of Dublin on the Board. The representatives of surgery and obstetrics have not yet died out, but we presume when the names of Colles, Fleming, and M'Clintock disappear from the list, they will be replaced by persons equally well acquainted with hospital management as the tradesmen who now replace Stokes and Corrigan on the Board. We have nothing whatever to say against the respectability of these gentlemen in their own walks of life, but we consider the Government offered a gross insult to the profession when the seats on the Board vacated by two eminent physicians were filled up by two individuals never before heard of as authorities on hospital management.

Lectures on Clinical Surgery. By JONATHAN HUTCHINSON, F.R.C.S. Vol. I. Part I.—*On Certain Diseases of the Skin.* London: J. & A. Churchill. 1878. Pp. 192.

THIS volume is one of a series which its accomplished author intends to produce on various departments of clinical surgery; and we feel sure that it may be accepted as a good earnest of the volumes which will, it is to be hoped, shortly succeed it. Some of the lectures were delivered several years ago, and have already appeared in print, but most are new, and all reprints have been carefully revised.

Every lecture well repays perusal; and even on special points (*e.g.*, the doctrine of varicella-prurigo), where the writer's arguments, although always ingenious, fail to carry conviction to our mind, it is impossible not to admire the combination of acute observation in details, with broad views and a wide range of illustration, which raises Mr. Hutchinson far above the level of a mere specialist. His cases are always interesting, and the tabular records given—for example, in relation to prurigo—testify to Mr. Hutchinson's unwearied industry and perseverance in note-taking. Great attention, too, is given to therapeutics; and in Lecture IV. a strong body of evidence is adduced to show that arsenic is a specific for the state of health upon which relapsing pemphigus depends.

We cannot doubt that this first instalment of the projected series will command a wide circle of readers; and, for ourselves, we may say that we have derived more enlightenment and received more suggestions for study from the writings of Mr. Hutchinson

than from those of any other writer upon diseases of the skin. We have not space to criticise the lectures in detail, and will conclude this notice with a sample of Mr. Hutchinson's powers of terse and lucid description. Speaking of the terms which have been applied to the varieties of ichthyosis, he says (p. 168):—

“ I am anxious that you should clearly understand that all these names are applicable to mere varieties of one and the same disease. The sameness consists in, and is proved by, the general conditions under which it is met with. Invariably beginning in early childhood, and usually present at birth, independent of all known causes of ill-health, prone to be general, liable to affect more than one in the family, affecting with tolerable uniformity certain definite regions—such are the clinical features which unite all the varieties of ichthyosis under one name. They far outweigh the little differences observed in different cases as to the anatomico-pathological products. We can afford to let the papillæ take a larger share in some cases than in others, or the sebaceous system to preponderate in some, without disturbing the conclusion that the basis malady is the same. A further very strong argument for identity is found in the fact that very often these different conditions are, as I have already said, met with on the same skin at different parts.”

Naval Hygiene. By JOSEPH WILSON, M.D.; Medical Director, U. S. Navy. Second Edition. Philadelphia: Lindsay & Blakiston. Pp. 274.

DR. WILSON'S is a most discursive and readable work; it contains a vast amount of information, given in the “ Sandford and Merton ” style, and we are certain that the author would be a popular favourite in enlivening the chit-chat of a naval mess-table. The plan of the work is based on a cruise of the writer's own, the various incidents of which serve as starting-points for most varied disquisitions. The preface states that it is written chiefly in the hope of proving useful to the captains of ships on long voyages, but we fear that these are just the people who are least likely to be lured by chapters on polypetalous exogens or poetical quotations on nostalgia. A chart of simple directions, categorically drawn up, would be far more effectual. However, we are bound to add that the author evidently possesses a vast fund of information on the subjects on which he writes, and that the care with which his book is compiled is worthy of all praise.

PART III.

HALF-YEARLY REPORTS.

REPORT ON MIDWIFERY AND DISEASES OF WOMEN.

By ARTHUR VERNON MACAN, B.A., M.B., M.Ch., M.A.O., Univ. Dubl.; F.K.Q.C.P.; Gynæcologist to the City of Dublin Hospital; Ex-Assistant Physician to the Rotunda Lying-in Hospital; Lecturer in Obstetrics at the Carmichael Medical College; and Examiner in Midwifery at the King and Queen's College of Physicians in Ireland.

THE BEARINGS OF CHRONIC DISEASE OF THE HEART UPON PREGNANCY, PARTURITION, AND CHILDBED.

It is hard to understand how this subject has so long remained almost wholly unnoticed by writers on obstetrics. Its importance is great, and the combination must be of frequent occurrence; yet till quite lately there has been no systematic attempt to investigate the mutual effects of heart disease and pregnancy on each other. Dr. Angus Macdonald has just published a most interesting monograph,^a in which the whole subject is treated in a masterly and exhaustive manner, and of which we can give but a very imperfect *résumé* in the limited space at our disposal in this report.

At the very threshold of the investigation we are met by the question—What is the physiological effect of pregnancy on the healthy heart? After a careful and critical review of all that has been written on this subject—including the investigations and experiments of Larcher, Ducrest, Duroziez, Fritsch, Löhlein, together with the sphygmographic investigations of Mahomed, Meyburg, Marey, Blot, and those he has himself instituted—he comes to the conclusion that the heart becomes physiologically hypertrophied to a slight degree during the latter months of preg-

^a "The Bearings of Chronic Disease of the Heart upon Pregnancy, Parturition, and Childbed." By Angus Macdonald, M.A., M.D., &c. London: J. & A. Churchill. 1878.

nancy. "There is, therefore, reason to believe that in the left ventricle, during pregnancy, we have as a physiological condition a certain degree of increase in its capacity, associated with a variable amount of muscular hypertrophy in its walls; and in the other chambers we have the same slight dilatation uncompensated by muscular hypertrophy" (p. 22).

To Professor Carl v. Hecker, of Munich, belongs the credit of being the first who drew the attention of the profession to the effect of pregnancy on previously-existing heart disease. He published, in the year 1860,^a two cases of heart disease, one of which proved fatal during delivery, the other twenty-three days after the confinement. His theory is that, "The danger to women who suffer from valvular lesions, which is determined by pregnancy and parturition, appears to me to be capable of developing itself specially in two directions. In the first place, during the latter months of the pregnancy, such a narrowing of the thoracic space is produced that the lungs, already embarrassed in their functions by the cardiac lesion, become, at times quite acutely, functionally incapable through serous effusion, and life is thereby brought to a standstill; or the heart is so used up by the exertions of the labour that its already disturbed mechanism is brought completely to a standstill, and thereby an end is made of life." This explanation appears to be, at first sight, a very plausible one; but it is founded on the idea that the vital capacity of the thorax is very seriously diminished by pregnancy, which, as Dr. Macdonald very clearly shows, is certainly not the case. The next author who published anything on the subject was Spiegelberg.^b He holds that immediately after labour there is a great decrease in the arterial tension, and a corresponding increase in the venous pressure. "These unfortunate accidents are," he says, "rather conditioned by this—that the degree of the compensation of the cardiac lesion being, under ordinary circumstances, enough to make the situation tolerable, is no longer sufficient for the altered, and especially for the suddenly altered, relations of pressure,"—the chief cause not being the change in the vital capacity of the thorax, but "the altered pressure under which the heart acts during the pregnancy in comparison with the condition after labour." Spiegelberg tries in this way to explain the fact that he had himself observed—viz., that in cases where there is aortic regurgitation the serious symptoms

^a Hecker and Buhl. *Klinik der Geburtakunde*. Leipzig. 1861. P. 173.

^b Archiv f. Gynaekologie. B. II. S. 236.

usually disappear immediately after labour, whereas in cases of mitral insufficiency or stenosis the most serious symptoms may not become developed till after labour.

Fritsch, in his papers on this subject,* also holds that the sudden accidents that arise from heart disease during childbed are due to the inability of the heart to accommodate itself to the sudden change in the relative arterial and venous tension. He thinks that the arterial tension must be increased rather than diminished (as Spiegelberg holds) by the closure of the uterine circulation. After labour the intra-abdominal tension is much lessened, and the blood that is forced out of the uterine circulation remains in the large venous trunks and in the abdominal venous plexuses, and too little blood reaches the dilated right ventricle. This, therefore, contracts feebly and irregularly, the amount of blood passing through the lungs is greatly diminished, the blood throughout the body becomes imperfectly oxygenated, vital force sinks, and, as a consequence, the cardiac muscle grows weaker and weaker. He agrees with Spiegelberg that the most serious cases are those where there is advanced mitral stenosis, though he differs from him entirely as to the explanation of its mode of action. Dr. Macdonald, finally, notices the views of Löhlein, who thinks that the serious symptoms in such cases are chiefly due to the upward pressure of the diaphragm during pregnancy, and to the increased flow of blood to the chest after delivery. He sums up this critical review as follows:—

“It will be seen that all the authors referred to are agreed that pregnancy is likely to introduce serious complications into the condition of a patient who suffers from chronic heart disease, except the lesion is of not very recent origin, and is well compensated, when there is a considerable probability that all may go well during the pregnancy, delivery, and childbed period.

“It also appears that the symptoms of evil omen arise chiefly in the region of the lesser circulation, present themselves in the form of extreme dyspnoea, suffocative bronchial catarrh, œdema of the lungs, etc., are exceedingly apt to be complicated by the spontaneous onset of premature labour, and frequently end in death either during delivery or the childbed period.

“There is likewise manifest agreement in the belief that those urgent symptoms seldom occur till the first half of the pregnancy is past, and that they increase as the period of utero-gestation advances, provided

* *Archiv f. Gynaek.* B. VIII. and X.

premature labour does not come on; but that it by no means follows that they will either abate or disappear when the labour is past, although they are likely to attain a special intensity during the process of delivery, more especially during the second stage.

“It will also be noticed that cardiac lesions of recent date are shown to be peculiarly dangerous, partly because, owing to the imperfect compensation attending them, they are apt to give rise to dangerous weakness of the right ventricle during pregnancy, and more especially during delivery; and partly because they are most apt to be associated with acute inflammatory changes in the endocardium, including ulcerative endocarditis. These conditions may appear either during pregnancy or after delivery, even though the urgent symptoms during labour may have been recovered from.

“As to the precise manner in which lesions of the heart lead to dangerous consequences in connexion with labour, Spiegelberg has thrown out the ingenious theory that it is owing to suddenly-diminished aortic pressure at the moment of delivery. The removal from the systemic circuit of the placental circulation, and the passage of the blood contained in the uterus before its final contraction into the venous circulation, according to him, have the effect of lessening suddenly the arterial tension and increasing the venous, more especially in the right side of the heart. If, then, the heart is already defective, and its compensation inclining to give way, this final assault is apt to so completely perturb its action as to lead to sudden death.

“Fritsch, on the other hand, believes that the danger under such circumstances is apt to arise rather from the dilated right heart being left too empty of blood, the large parametric venous plexuses, the other abdominal veins, &c., affording abundance of space to accommodate a great quantity of blood, especially after delivery, and when the abdominal pressure is not so great as it ought to be.

“This condition of matters is apt, according to him, to lead to a sudden gush of blood from the abdomen into the empty right side of the heart, and to exert there a paralysing effect upon the organ, explained, as he believes, by Panum's experiment, which shows that a sudden gush of blood into the empty heart does paralyse that organ. The right heart then begins to contract upon the too small quantity of blood contained in it, its action is weak and irregular, too little blood gets to the pulmonary circulation; the blood is consequently imperfectly aërated. This, again, leads to imperfect cerebral nutrition, and so a vicious circle is established which ends in death.

“Löhlein, again, denies both these views, and refers those accidents in general to imperfect cardiac compensation, aggravated by compression of the chest caused by the distended abdomen during pregnancy, and after

delivery by excessive blood-supply to the lungs, occasioned by the aspiratory effect of the over-distended thorax. In certain other cases—and more frequently, he thinks, than is usually suspected—he believes that acute inflammation of the diseased valves may lead to serious aggravation of symptoms both during pregnancy and childbed.”

He then gives at great length the histories of 31 cases of heart disease complicating pregnancy and lying-in. Of this number, 13 were more or less under his own immediate observation; the remainder are collected from English and German literature. From a number of carefully-prepared sphygmographic tracings he shows very conclusively, we think, that during pregnancy, especially the latter months, arterial tension is increased, and that it is only slightly diminished during the lying-in period. The mortality in these 31 cases was 17, or 55 per cent., but this, he thinks, is probably too high. In severe cases the pregnancy is almost always interrupted, and in cases of aortic regurgitation the serious symptoms seem to cease suddenly as soon as labour is terminated. If, however, there is advanced mitral disease, there is almost as great danger of death taking place during convalescence as during labour itself; and stenosis would seem to be much more dangerous than simple insufficiency. Indeed, Dr. Macdonald does not think that a favourable prognosis can be given in cases of mitral disease for at least three weeks after labour. The most distressing subjective symptoms are dyspnoea, with a sense of suffocation, præcordial anxiety, and in aortic cases a marked tendency to syncope during labour. He thinks that premature labour should not be induced, except in cases where the abdomen is unduly distended, as from hydramnios; further, that chronic heart disease, especially well-marked mitral stenosis or aortic incompetency, should be looked on as a grave contra-indication to marriage, and that persons so affected should in no case suckle their children. The treatment of heart disease during pregnancy should be conducted on the same general principles as guide us in such cases when not so complicated. So far from heart disease being a contra-indication to the administration of chloroform during labour, it is, according to Dr. Macdonald, exactly the reverse—the effect of chloroform being beneficial, from its power of lowering the arterial tension. All undue delay in the second stage should be avoided by the timely use of the forceps, or, in suitable cases, by version, and the membranes should be ruptured early when hydramnios is present.

CÆSAREAN SECTION COMBINED WITH EXTIRPATION OF THE
UTERUS AND OVARIES. (PORRO'S OPERATION.)

Since this operation was first proposed and practised by Prof. Ed. Porro, in 1876, a number of cases have been reported by other operators, and Dr. A. Breisky has lately published a most interesting paper on the subject in the fourteenth vol. of the *Archiv für Gynaekologie*. Out of six cases collected by him, and one case of his own, five mothers have recovered. That this is a most favourable result, compared with that after the ordinary operation of Cæsarean section, may be judged from the fact that Spaeth, of Vienna, was unable to find the record of a single successful case of Cæsarean section in the records of the Vienna Lying-in Hospital, though he went back to the time of Boër, and that there has not been a successful case in the hospital in Prague since the year 1844.

The operation was performed by Dr. Breisky on a patient with a universally-contracted, flat rachitic pelvis, having a diagonal conjugate of 7·9 cm. and a conjugata vera that Breisky estimated at from 6·2-6·7 cm. The operation was performed with every possible antiseptic precaution, and the incision through the abdominal walls was commenced 3 cm. above and to the left side of the umbilicus, and extended to within a finger-breadth of the pubis. Through this opening the uterus was with very great difficulty drawn, and then Breisky passed two copper wires round the supra-vaginal portion of the uterus, and tightened them with a pair of Cintrat's serre-nœuds. He then opened the fundus uteri by a free incision, which implicated the insertion of the placenta for almost its entire extent. He therefore immediately detached the placenta, and extracted the foetus as quickly as possible by the feet. Thanks to the copper-wire ligatures the hæmorrhage from the wound in the uterus and from the insertion of the placenta did not exceed the quantity usually lost in a natural confinement. The child was slightly asphyxiated, but after some little trouble it commenced to breathe regularly. The uterus and its appendages were now removed about 2·5 cm. above the ligatures, but in an effort which was subsequently made to tighten these one broke, and the second slipped off the stump, and for some time there was troublesome hæmorrhage, some blood finding its way into the abdomen. Two fresh wires were soon substituted and the hæmorrhage effectually controlled. The peritoneum was then thoroughly cleansed and the abdomen closed by eight silver and ten silk sutures. The stump of

the uterus was fixed in the lower angle of the wound, and three drainage tubes introduced immediately above it, the whole being covered with Lister's dressing. The bowels were moved by an enema on the fifth day, the copper wires became quite loose on the seventh, and on the twenty-third day the patient was up and walking about.

This operation, Breisky thinks, marks an epoch in the history of the Cæsarean section, and to Porro is due all the credit of introducing it. Breisky himself adopted in this case the modifications of the operation proposed by Dr. P. Müller—viz., to draw the uterus out of the abdomen and to pass the ligature round the supravaginal portion before proceeding to make the incision into the fundus. By this method not only is all danger of hæmorrhage prevented, but there is thereby less chance of blood or septic matter gaining entrance to the abdomen. It is by no means always easy to draw the uterus out through the abdominal wound, and in order to lessen its size the membranes should be ruptured before the operation is commenced. The great advantages of applying the ligature before incising the uterus is well seen in this case, in which the incision involved the insertion of the placenta for the greater part of its extent. It also enabled us to prolong the incision well into the fundus and to leave the cervical portion untouched, thus insuring a long stump. Breisky does not approve of gradually tightening the wires during convalescence, as proposed by Porro and Spaeth, except when there is danger of secondary hæmorrhage. In order to avoid any chance of septic infection after the operation he recommends that the dressings should be changed twice daily.

LAPARO-ELYTROTOMY—A SUBSTITUTE FOR THE CÆSAREAN SECTION.

In the *American Journal of Obstetrics* for April, 1878, Dr. T. Gaillard Thomas published a paper in which he urges the adoption of this operation as a substitute for the Cæsarean section. The operation consists in opening into the roof of the vagina through an incision in the abdominal walls reaching from the spine of the pubis to the anterior spine of the ilium. Through this incision the hand is introduced into the uterus, and the child turned and extracted. This operation was first proposed by Jörg in 1806, and was afterwards modified by v. Ritgen. Up to the year 1870, when Dr. Thomas revived the operation, it had been but once performed, and that unsuccessfully, by Ritgen himself. Thomas gives the

results of five cases; in three of them both mother and child were saved, while in a fourth the child, which was premature, was born alive, and lived for an hour.

Dr. Thomas thus describes his first operation on the living body (*Am. Jour. of Obstetrics*, April, 1878, p. 231):—

“The patient being placed upon a table, anæsthesia was produced, to the point of quieting her restlessness and jactitation, with a few inhalations of ether. I then passed my hand up the vagina and dilated the cervix slowly and cautiously, so that at a three-quarter distension no injury was done to its tissue. With a bistoury I then cut through the abdominal muscles, the incision being carried from the spine of the pubis to the anterior superior spinous process of the ilium. The lips of the wound were then separated, and by two fingers the peritoneum was lifted with great readiness, so that the vagino-uterine junction was reached. The vagina was then lifted by a steel sound passed within it, and cut, and the opening thus made was enlarged by the fingers. The cervix was then lifted into the right iliac fossa by the blunt hook, while the fundus was depressed in an opposite direction. I then passed my right hand into the iliac fossa, and introduced two fingers into the uterus, while the left hand, placed on the outer surface of the uterus, depressed the pelvic extremity of the foetal ovoid. The knee was readily seized, and delivery easily and rapidly accomplished. The child was born alive, but was a badly developed, hare-lipped, and, as I before stated, premature infant. It lived for an hour or two, during which time the rite of baptism was administered to it. The mother, the wound in whose abdomen was closed by interrupted suture, died about the same time as the child.”

This operation was performed for the first time in Europe on July 14, 1878, by Dr. T. Whiteside Hime, of Sheffield, who has given a full account of the case in *The Lancet* of Nov. 9th, 1878. The woman was suffering from advanced cancer of the recto-vaginal septum, which had contracted the vagina to about two inches. The upper part of the posterior wall of the vagina and the interior wall were soft and elastic, and the uterus was healthy. The operation was performed in direct accordance with Dr. Thomas' directions, and a living child was turned and delivered without difficulty, the placenta being expelled spontaneously. This patient was very unruly after the operation, and died in about two hours, as Dr. Hime thinks, from cerebral anæmia. At the *post mortem* examination it was found that neither the bladder nor the peritoneum had been in the least injured. Dr. Hime is convinced that this operation will entirely supersede the Cæsarean section, and perhaps even craniotomy.

WHEN SHOULD WE LIGATURE THE UMBILICAL CORD?

A great deal has been written of late on this subject—the question being first raised by Dr. Budin in a series of communications to *Le Progrès Médical* for 1875–76, in which he stated that tying the cord immediately after the child is born deprives the latter on an average of 92·6 grammes of blood (more than 6 ozs.), which it would have received from the placenta if the ligature had not been applied till two or three minutes after all pulsation in the cord had ceased and the child had cried out lustily. Since the publication of these papers a number of experiments have been made with a view of proving or disproving this very startling statement. Schücking published the results of his observations in the *Berlin. klin. Wochenschrift*, 1877, Nos. 1 & 2. He thinks that almost the whole of the blood that is contained in the foetal portion of the placenta is finally transferred to the infant. This transfer is effected by the pressure exerted by the uterine contractions on the placenta, and not by any aspiration caused by the expansion of the infant's thorax. Schücking estimated the amount of this "reserve blood," as he calls it, at from 70–150 grammes, and the time requisite for the transfer varies from a few to several minutes, being determined by the amount of pressure exerted by the uterus on the placenta. Hence he argues that unless we wish to deprive the foetus of nearly half of its proper supply of blood, we will not apply the ligature to the cord till some minutes after the child has been born; and if from any cause, such as *post partum* hæmorrhage, we are obliged to press off the placenta immediately, we should afterwards expel the blood from the placenta into the foetal circulation by compressing the placenta between the hands. He, at the same time, protests most strongly against treating the asphyxia of newly-born children by allowing some hæmorrhage to take place from the cord. This treatment is founded on the supposition that the child's heart is already too full of blood, which must be got rid of at any price. This idea, Schücking thinks, is quite erroneous. For at the first effort the infant makes at inspiration the blood rushes into the thorax, leaving the extra-thoracic vessels empty. These are then filled by the "reserve blood" from the placenta; now if we tie the cord quickly and cut off this supply of "reserve blood," while at the same time we allow some blood to escape from the foetal end of the cord, we increase the anæmia, and, as a natural consequence, lessen the reflex sensibility of the medulla. As a direct consequence of

this the intervals between each effort at inspiration become longer, till finally the breathing stops altogether.

With a view of still further elucidating this question, Prof. Zweifel, of Erlangen, instituted a number of experiments to determine the exact quantity of blood that remains behind in the placenta when the cord is tied immediately after birth, and also when some minutes are allowed to elapse before the ligature is applied.* He found that the average quantity of blood remaining behind in the placenta when the cord was tied immediately after the child was born, was 192 grammes; but, when the cord was not ligatured till after the placenta had been pressed off by the hand, the average amount of blood contained in the placenta was only 92·29 grammes. In other words, when the usual method is followed—viz., tying the cord as soon as all pulsation has ceased in it, and the child has cried out lustily—the child is deprived of 100 grammes of blood which it would have if the ligature were not applied till after the placenta had been pressed off. It is also well known that all children lose weight for some days after birth, the amount lost being estimated at an average of 220 grammes; but Prof. Zweifel found that the average amount of this loss, when the ligature had not been applied till after the expulsion of the placenta, was only 156 grammes.

Dr. Leopold Meyer,^b of Copenhagen, has repeated the experiments of Prof. Zweifel, but has arrived at very different conclusions. He found the following results in five cases when the cord was tied late—i.e., after the expulsion of the placenta:—

	Weight of the Placenta.	Blood contained.	
1.	502 gr.	70·34 gr. or 14·01 per cent.	} Average, 15·07 per cent.
2.	527 gr.	85·5 gr. „ 16·21 „	
3.	600·5 gr.	104·36 gr. „ 17·38 „	
4.	426·5 gr.	56·41 gr. „ 13·23 „	
5.	496 gr.	72·04 gr. „ 14·52 „	

The other cases, or those where the ligature was applied early, he divides into two classes—(a) where the ligature was not applied till after the cessation of the pulsation in the cord, and (b) where it was applied as soon as the children were born.

In three cases of class (a) the results were as follows:—

	Weight of the Placenta.	Blood contents.	
6.	737·5 gr.	96·69 gr. or 13·11 per cent.	} Average, 17·25 per cent.
7.	458·5 gr.	79·71 gr. „ 17·39 „	
8.	600 gr.	127·57 gr. „ 21·26 „	

* Centralblatt f. Gynaekologie. 1878. P. 1.

^b Ibid. P. 220.

And in the same number of cases of class (b):—

	Weight of the Placenta.	Blood contents.	
9.	610 gr.	125·4 gr.	or 20·56 per cent.
10.	494·5 gr.	91·93 gr.	„ 18·59 „
11.	657 gr.	102·6 gr.	„ 15·62 „
			} Average, 18·26 per cent.

If we take the average of the last six cases we get 17·76 per cent., making the difference between the cases ligatured late and those ligatured early only 2·69 per cent. of the weight of the placenta; or if we take the average weight of the placenta as 600 gr., this would give 16 gr. of blood in favour of the cases that were ligatured late. Thus, though he agrees with Zweifel that the foetus receives more blood if the cord is tied late, he differs most seriously with him as to the exact amount thus gained.

In the *Centralblatt für Gynaekologie*, 1878, p. 409, Dr. M. Hofmeier, of Berlin, draws attention to the *à priori* improbability of Zweifel's statements—viz., that by the ordinary method of ligaturing the cord the child loses 100 grammes of blood, the total amount of blood in an average child of 3,300 gr. being only 175 gr. Through the goodness of Prof. Schroeder he was enabled to make some experiments with a view of throwing some light, if possible, on this subject. The method adopted was to place the child the moment it was born, and before the cord was tied, upon a sensitive weighing machine, to note its weight then, and also the increase or diminution in its weight after some minutes had elapsed. The number of cases in which he made the experiment was 32, and the result was that there was an average increase in weight which amounted to 63·6 gr. This increase in weight, he considers, cannot be due to anything but the extra amount of blood that has during the interval entered the foetal circulation from the placenta.

A more difficult question to answer is—What benefit or use is this immense quantity of blood to the foetus? It is either superfluous, in which case it is soon got rid of by disintegration of the red blood corpuscles and absorption of the serum; or it is most useful, in which case it must be looked on as “reserve blood,” and as such must tend to lessen the amount of loss of weight which such children would otherwise suffer after birth. He concludes, from the results of a number of weighings that were undertaken with the object of answering this question, that children whose cord was ligatured late—i.e., after the expulsion of the placenta—lost 1 per cent. less of their entire weight than other children. This in a child weighing 3,300 gr. would amount to 33 gr., which represents a very considerable increase of blood and strength; and

he has found, moreover, that such children begin to gain flesh from $\frac{1}{3}$ – $\frac{1}{2}$ a day sooner than the others.

Dr. Max Wiener publishes the results of some experiments that he has made on this subject in the *Archiv f. Gynaekologie*, B. xiv., p. 34, which in the main agrees very closely with those obtained by a very similar method by Meyer. He remarked in the course of his experiments that the quantity of blood that remained in the placenta was very variable even in the same class of cases. This difference could not be put down to the time that elapsed before the cord was tied, nor to the amount of uterine contraction, nor to the development of the child. He thinks, therefore, that this great difference in the amount of blood that remains behind in the placenta in almost similar cases must depend on the different ratios that often exist between the size of the child and the size of the placenta, and therefore between the amount of blood in the child and that contained in the placenta. Thus we may find two well-developed children each weighing 3,000 gr., while the placentæ weigh 600 and 400 gr. respectively, and consequently the amount of blood found in each will be proportionate to the size of the placenta and not to the weight of the children.

Dr. Leopold Meyer publishes some further observations on this subject in the *Centralblatt für Gynaekologie* for April 26, 1879, which lead to conclusions directly opposed to those of Hofmeier. The latter found that children whose cord was tied late lost one per cent. less of their total weight during the first few days of their existence, and that they began to gain weight from $\frac{1}{3}$ – $\frac{1}{2}$ a-day sooner than those whose cord was tied immediately after birth. Meyer has weighed 40 children—in 20 of them the cord was tied early and in 20 late. The average weights are as follows:—

					Cord tied late.	Cord tied early.	Difference.
Average weight after birth,					3,203 gr.	3,268 gr.	65 gr.
1 day	-	-	-	-	3,128	3,188	60
2	-	-	-	-	3,013	3,069	56
3	-	-	-	-	2,990	3,085	95
4	-	-	-	-	3,054	3,134	80
5	-	-	-	-	3,094	3,192	98
6	-	-	-	-	3,141	3,233	92
7	-	-	-	-	3,181	3,287	106
8	-	-	-	-	3,221	3,310	89
9	-	-	-	-	3,261	3,353	92
10	-	-	-	-	3,277	3,383	106

The difference here shown in favour of the cases where the cord was tied early depends, Meyer thinks, on the original weights of the children; and if all children who weighed above 3,500 gr. be left out of consideration the results in both series are almost exactly identical. Hence he concludes, contrary to the opinion of Porak, Budin, Schücking, Zweifel, Hofmeier, and Riebemont, that the time that is allowed to elapse between the expulsion of the child and tying the cord has no effect—or, at all events, in comparison to other influences, has next to no effect—on the subsequent weight of the child. He further found, contrary to the results that Hofmeier brings forward, that a child whose cord was tied early did not subsequently lose so much of its weight as one whose cord was ligatured late. Hence tying the cord late does not increase to any great amount the quantity of blood in the foetal circulation, and the results that have been obtained by weighing children immediately after birth and then again a few minutes later are founded, he thinks, on some error due to traction on the cord or some other cause.

In France this question has also been keenly debated ever since Dr. Budin first published the result of his investigations. Dr. Ch. Porak contributed a most exhaustive article on the subject to the *Revue Mensuelle de Méd. et de Chir.* for May and June, 1878. He agrees with Dr. Budin as to the amount of additional blood that enters the infantile circulation when the cord is tied late, but thinks that this extra blood, far from being any advantage to the infant, as Dr. Budin thinks, is rather positively injurious; for such children are, he says, much more subject to infantile jaundice and to the various effects of plethora, such as hæmorrhage from the stomach, bowels, and vagina, and he adduces cases in support of this idea.

The *Annales de Gynécologie* for February, 1879, contain a paper on this subject by Dr. Alban Riebemont, the value of which, from a scientific point of view is, however, greatly lessened by the violent polemic tone that pervades it throughout. He takes Dr. Porak very severely to task not only for his facts, but also for his logic and conclusions. He sums up his paper as follows:—

1. By ligaturing the cord late the infantile circulation receives on an average an addition of 92 grammes of blood (Budin).

2. This blood, which is contained in the placental vessels, is most necessary for the full establishment of the infantile circulation.

3. The blood is drawn into the infantile circulation chiefly by the suction power exerted by the expansion of the chest walls

(Budin), the pressure exerted by the uterus on the placenta (Schücking, Porak) having no considerable effect.

4. In cases of asphyxia where the child has a bluish hue the cord ought not to be immediately tied, nor should any hæmorrhage be permitted from its foetal extremity.

5. Ligaturing the cord late does not expose the child to the smallest immediate or ulterior danger.

6. The infant is thereby placed in the most advantageous circumstances possible for its development; it loses less weight, and regains what it has lost both sooner and quicker than if the ligature be made immediately.

7. The expulsion of the placenta is thereby rendered easier, and there is less resistance offered to its escaping through the cervix (Budin, Schücking).

8. He agrees with Hofmeier, Zweifel, Schücking, and Budin that the cord should not be tied till the pulsation in it has entirely ceased.

EMMET'S OPERATION FOR LACERATION OF THE CERVIX UTERI WITH EVERSION OF THE LIPS OF THE OS.

In the Report on Obstetrics that appeared in this Journal in November, 1875, a short abstract was given of a paper by Dr. Thomas A. Emmet, in which this author described the above affection and the operation he has devised for its radical cure. Since the publication of Dr. Emmet's paper the operation has been very extensively performed in America by Drs. Pallen, Skene, Goodell, Mundé, Wing, Baker, Dudley, as well as its original proposer, and has also been adopted by some of the leading gynæcologists on the Continent; but as far as we are aware it has not yet been performed to any extent in this country. Some adverse criticism which it has lately met with in America has led Dr. Mundé to publish an able article in its defence in the *American Journal of Obstetrics* (January, 1879), of which he is the editor. In this paper he draws the attention of the profession to the fact—so strongly insisted on by Dr. Emmet—that a very large majority of cases that are at present known and described as ulceration of the os are not cases of ulceration at all, but are the result of laceration of the cervix uteri with eversion of the lips of the os, and subsequent granular hypertrophy of the surfaces thus exposed. The diagnosis of such cases is difficult through the ordinary Ferguson's speculum, but is very easy if Sim's instrument be used—a hook inserted into the anterior and posterior margins of the

so-called ulcer, and then the two hooks approximated. When this is done the two ulcerated surfaces roll inwards and the natural outline of the cervix becomes restored. The operation consists simply in fastening the edges thus brought together with silk or wire sutures—union, in nearly every case, taking place by first intention.

This laceration of the tissues of the cervix is due to the child's head passing through the os before it is fully dilated, and occurs to some extent in almost all first cases. But unless the laceration be very extensive, or unless union be prevented by some inflammatory puerperal disorder, eversion does not subsequently take place. As the disease has hitherto been well known under the name of ulceration of the os, it is hardly necessary to describe the symptoms. Dr. Mundé does not deny that the lesser degrees of the affection may be cured by the treatment usually adopted for ulceration—such as the application of strong caustics, vaginal injections, and pessaries, joined with perfect rest and quietness. But such treatment must be continued for months, and even then relapses are of frequent occurrence. These cases, he asserts, can be cured with certainty in a couple of weeks by Dr. Emmet's operation. Another of the advantages that he claims for it is, that it acts as a stimulus to the nutrition of the uterus, in the same way that amputation of a portion of the cervix does, and hence when the uterus is greatly enlarged from chronic areolar hypertrophy, more commonly known as subinvolution, the operation is followed by a great reduction in the size of the uterus. Its highest recommendation is, however, that it tends to prevent the future occurrence of malignant disease of the cervix, which has of late been looked on by many gynecologists as originating in increased growth of the glandular and papillary structures from constant friction and irritation. This is entirely prevented if the cervical mucous membrane is again turned inwards and the outline of the cervix restored. Dr. Mundé has met with 119 cases of laceration of the cervix of sufficient extent to demand treatment among the 700 women who applied at his hospital during the last two years for uterine complaints; and in order to show the frequency of this affection is not exceptional, Dr. Goodell, in which he says:—"My *ov* Dispensary for Diseases of Women, at the *sylvania*, would lead me to infer that about women suffering from uterine trouble has an the cervix."

This paper is illustrated by twelve coloured drawings, done by Dr. A. H. Fridenberg, which are the most striking and truthful representations of so-called ulceration of the os that we have ever seen, and which reduce the proof of Dr. Mundé's assertion to a simple ocular demonstration.

This paper of Dr. Mundé's has elicited a letter from Dr. Pallen (*Am. Jour. of Obstetrics*, April, 1879), who was the first person who published a case of eversion treated by operation, in which he quotes extensively from a paper published by him in 1874, entitled "The Accidents of Parturition requiring Surgical Treatment." We must content ourselves with giving that part only which refers to the operation itself:—

"The operative procedure is here quite simple. It consists of complete denudation of the cicatricial and ulcerated (papillary hypertrophy) margins, and their approximation (as in hare-lip), and retention by means of silver sutures. Annealed iron-wire is possibly as good. The difficulties of the operation consist in thoroughly freshening the edges about the upper angle of the fissure; but to accomplish this the surgeon must be provided with scissors and cutting-pliers of all shapes, so that the most tortuous and sinuous track may be reached. The passing of the needles is somewhat troublesome, as the tissue of the cervix is dense and tough, and the space for action quite limited. Short, stout, and straight needles, not more than five-eighths or three-quarters of an inch in length, *trocár pointed*, are passed very easily. These needles should be armed with a loop of fine, strong silk thread, not waxed and without any knot, to which is attached a tapered silver wire. As many sutures are passed as the surgeon thinks proper, but certainly one to every three lines of surface-denudation."

The operation is very rarely, indeed, followed by any reaction. Dr. Mundé has operated in his own office without ether, and when the operation was finished the patient has gone home in the tramway.

We regret that we were unable to get access to Dr. Goodell's paper, which Dr. Mundé says contains the most forcible and graphic description he has read of the peculiar appearance and consequences of this lesion.

We entertain little doubt that Dr. Mundé will soon be no longer able to quote the Dublin School of Gynæcology as refusing to recognise the true etiology of these cases of eversion of the cervix, or the importance of the operation that has been so ably devised by Dr. Emmet for their permanent cure.

PART IV.
MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE
COLLEGE OF PHYSICIANS.

SESSION 1878-9.

HENRY H. HEAD, M.D., President.

GEORGE F. DUFFEY, M.D., Honorary Secretary.

Wednesday, May 7, 1879.

PROFESSOR WILLIAM MOORE, M.D., in the Chair.

Case of Lupus.

DR. WALTER SMITH introduced a patient suffering from lupus, and explained a recent method of treating that disease. It was proposed by Volkmann, of Halle, and had been named *eration*. Volkmann had devised spoons to be used for scraping the rotten lupous tissue away, and a modification of these had been recently brought forward by Mr. Balmanno Squire, of London. In the patient now presented the disease was of twenty-five years' standing, and she was sent to him a few weeks ago by Dr. Rainsford to see if anything could be done for her. When she came to Dr. Smith her cheek was a mass of disease, being covered with thick laminated, shaggy crusts, and round the margin of each patch was the usual rim of gelatinous reddish nodules. Anyone who saw her a month ago would be struck by the change which has since been produced in her appearance. The instruments used in the treatment of her case were the scoops which he exhibited. The method used was to freeze the part with ether spray, and then scrape off the lupous tissue, and it was surprising how readily it came away, while no mischief was done to the healthy skin, which resisted an immense amount of scraping. No other treatment was used. Another remarkable point was the facility with which the hæmorrhage was stopped. Her face was dripping with blood after the operation, but the bleeding was stopped by the application of a

wet towel and blotting paper. In a few days after the operation the skin was filled up with healthy tissue. It would be seen that the patch where the disease had existed showed scarcely any mark of having been operated on, the cicatrix being on a level with the skin. The instrument combined facility with rapidity of execution, and there was freedom from pain after the operation. Mr. Squire's instruments are an immense improvement on those of Volkmann, which are much larger and clumsy in make. The patient before them had large tracts of lupous disease on her arms also. Dr. Smith also showed small knives used for multiple scarification in places where Mr. Squire's instrument could not be used.

A case of Phlegmonous Erysipelas of the Face ; Pyæmia ; Cerebral Embolism ; Pericarditis. By J. W. MOORE, M.D., Dubl. ; F.K.Q.C.P. ; Physician to the Meath Hospital and County Dublin Infirmary.

ON the afternoon of Good Friday, April 11, Matthew T., aged sixteen years, a farrier's apprentice, was admitted to the male epidemic ward of the Meath Hospital, suffering from a severe attack of phlegmonous erysipelas of the left side of the face. It was, as well as could be made out, the seventh day of his illness, the origin of which was rather obscure. Even at this early period his state caused great anxiety. The axillary temperature was 102.6° , his pulse was 144, and the respirations were 38 per minute. He was prostrate. There was enormous swelling of the lips, and the left cheek and side of the neck were brawny, dark coloured, and inclined to pit in places. A heavy foetor came from his breath. At night a gush of diarrhœa occurred. Next morning he seemed to be on the verge of cancrum oris. My colleagues, Dr. Foot and Mr. Ormsby, kindly visited him with me, and we all regarded his condition as most critical. The treatment consisted in the free administration of tincture of the perchloride of iron, with chlorate of potassium and quinine, frequent rinsings of the mouth with solutions of permanganate of potassium and chlorinated soda alternately, the application of an ointment of ferrous sulphate, vaselin, and glycerin to the affected part; and lastly, a nourishing dietary, with port wine and brandy. The ward was kept warm, and at the same time well ventilated, a carbolised steam being diffused through the atmosphere by means of one of Allen's patent kettles.

During the following four days, the erysipelatous inflammation spread over the right side of the face, the eyes at last being completely closed. His strength, however, seemed to hold out well until the twelfth day, when he complained of intense rheumatoid pains in the joints and along the limbs. He also coughed frequently, and auscultation and percussion revealed the presence of a decided bronchial catarrh, as well as of hypostatic congestion or extensive hæmorrhagic infarctions in both lungs. He was now put on five-grain doses of salicylate of ammonia given at short intervals. The chest was dry-cupped and kept poulticed. Mr.

Middleton, the clinical clerk, frequently tested the urine for albumen from this time forward, but with negative results. A rapid rise of temperature to 104.8° on the evening of the 16th day was followed by the appearance of red patches here and there over the body. These soon developed into unhealthy-looking pustules, which in time dried up, leaving blood extravasations under the cuticle. The skin now assumed a sallow or dusky ictteroid tinge. From the 15th to the 22nd day the temperature curves resembled a tertian ague.

Notwithstanding his desperate state, the patient seemed to be weathering the storm of disease to which he was exposed; when, at 5 a.m. of Saturday, April 26th—the 22nd day of his illness—he was seized with a sudden and violent epileptiform convulsion. This was succeeded by collapse or coma, from which he with difficulty rallied after some hours. He was then found to be completely hemiplegic on the left side, including the face. We ventured to make the diagnosis of pyæmic embolism of the right middle cerebral artery—basing our opinion on the remarkable history of the case and the sudden occurrence of left hemiplegia. A turpentine enema was at this time administered, which brought away a large formed motion and relieved distressing tympanites.

Little need be said of the subsequent course of the case—the temperature again rose, the heart failed, the pulse ran up to 150 or 160, there was cerebral respiration with extreme action of the *alæ nasi*, and he died at 9 a.m. of April 30, on the 27th day of his illness and the 5th from the occurrence of hemiplegia.

The *post mortem* examination was made about three hours after death. There was scarcely any *rigor mortis*. The body was still fairly nourished. The skin was sallow, with localised subcutaneous extravasations of blood or of blood pigment. The area of præcordial dulness was enlarged, and, although pericardial friction had not been heard during life, we anticipated the finding of a considerable pericardial effusion and pericarditis. On opening the thorax, the anterior portions of the lungs seemed healthy; the pericardium was much distended with a slightly blood-stained serum. There were pleuritic adhesions at the lower and lateral parts of both pleuræ. Considerable portions of lung were carnified—a result, apparently, of hypostatic congestion; while smaller nodules of indurated pulmonary tissue seemed to mark the site of hæmorrhagic infarctions. These morbid changes were more pronounced in the right than in the left lung. There was well-marked recent pericarditis, with firm adhesions between the visceral and parietal surfaces of the pericardium near the apex of the heart. The left ventricle was firmly contracted (*systole*), but appeared healthy. All the valves were smooth, but at the base of the anterior curtain of the mitral valve and in the aorta immediately above the aortic opening there were a few patches of recent atheroma. The pulmonary artery was not minutely examined.

In the abdomen, the liver was displaced downwards to some extent and slightly enlarged. On the surface were some patches indicating a subacute perihepatitis. The spleen was unfortunately not examined. The kidneys were larger than normal. On section, they proved to be hyperæmic, and small-sized pyæmic infarcts were found in several situations, as well in the cortical substance as in the medullary portion of both organs.

On opening the calvarium a few collections of lymph were noticed in the neighbourhood of veins in the pia mater. The circle of Willis was healthy, with the exception of the right middle cerebral artery, and that portion of the right anterior cerebral artery lying between its origin and the anterior communicating artery. These vessels appeared as if they had been injected for purposes of dissection. They were occluded for a considerable distance by an embolus or thrombus. The parts of the anterior and middle cerebral lobes near the fissure of Sylvius, and supplied by the middle cerebral artery, were softened and reduced to a pappy consistence. These changes penetrated the brain substance to the neighbourhood of the right lateral ventricle, which was itself apparently quite healthy.

The CHAIRMAN remarked that he thought hemiplegia arose from embolic affections of the brain more frequently than from anything else.

DR. HENRY KENNEDY said it was worth considering whether this case could have been one of glanders. The boy was a farrier's apprentice, and the symptoms reminded him of three or four cases he had seen of that disease, in which there was the commencement of the disease in the face and the curious form of eruption. Whether this was a case of glanders or not, however, he did not take upon him to say.

SURGEON-MAJOR JACKSON, C.B., read a paper on "Notes on Cases of Fever treated in Cyprus." [It will be found at p. 480.]

The CHAIRMAN remarked that in fevers occurring in this country they were not accustomed to a temperature of 106° falling six or seven degrees at night and getting up again in the morning. The duration of fever in Surgeon-Major Jackson's cases was from ten to twelve days, so that the fever was apparently either bilious or intermittent.

DR. DUFFEY said the paper was peculiarly interesting to him, as he had served five years in the Mediterranean, and he thought that many of the cases of fever Surgeon-Major Jackson had met with in Cyprus appeared to be similar to a type of fever which he had seen at Malta during his service there. He wished to ask Surgeon-Major Jackson had he noticed any eruption in his cases? In the Malta continued fever the existence of a characteristic eruption was, to a great extent, exceptional, and if present, was apt to be overlooked, as it was sparse, and often only

on the back. He had seen several cases which followed the course the reader of the paper had so well described, and in such of these as proved fatal there was indubitable pathological evidence of the fever having been typhoid. Another point of similarity between the Malta and the Cyprus fevers was the very protracted convalescence. Though in many of the Malta patients the pyrexia ceased in the usual time, the ultimate recovery of the patient was protracted, and many of the cases had to be invalided to England. There were two very well-marked forms of fever in Malta—one being of an ephemeral, and the other of a continued character; but the former very often ran into the latter. During convalescence rheumatic pain occurred in nearly every case. One particular but rather unusual form of rheumatism which he (Dr. Duffey) had described affected the testicle. From his experience and study of the continued fever at Malta, it was his opinion that it was essentially enteric in type. He thought that probably some of Surgeon-Major Jackson's cases were cases of enteric fever also. Several of the symptoms mentioned by Surgeon-Major Jackson—*e.g.*, epistaxis, vomiting, diarrhoea, and hæmorrhage from the bowels, were characteristic of enteric fever. He (Dr. Duffey) had also seen in Malta cases presenting a peculiar yellow appearance of the skin, persistent after death, which event occurred, in two instances at least, in about forty-eight hours after admission to hospital, and on a *post mortem* examination the bodies exhibited the anatomical signs of enteric fever. That led him to ask the author of the paper had he examined the state of Peyer's patches in his fatal cases? Surgeon-Major Jackson had said that the spleen was in all the cases greatly congested, which was frequently the case in enteric fever both at home and abroad. No doubt enlargement of the spleen also took place in remittent fever. He wished also to ask had Surgeon-Major Jackson's attention been directed to the state of the mesenteric glands in his cases? One therapeutical argument against the malarial character of the continued fever in the Mediterranean was the utter uselessness of quinine in its treatment, as his friend, Surgeon-Major Boileau, had also pointed out.

DR. LAWLOR said he had understood Surgeon-Major Jackson to say that the discharges from the bowels of the patients were not bloody, but green. In the relapsing fever that they had in Ireland many years ago, he formed a very strong opinion that the green discharges were altered blood, and he was inclined to think that the green discharges mentioned by the author of the paper were of a similar character.

SURGEON-MAJOR JOHNSON said that on looking through the charts shown in connexion with cases now brought before them, it struck him that they showed that the treatment had been really successful, though Surgeon-Major Jackson had stated the contrary.

SURGEON-MAJOR JACKSON, in reply, said that only in a few of his

cases did he observe any eruption. There was merely a dusky look, but no mulberry rash, or anything resembling typhoid eruption. The convalescence was most protracted. A good many of the cases had not recovered yet, and he believed they never would. Very few of the patients had any rheumatic pains. During the acute stages of the disease they had nervous pains, either in the spine or shooting down the back and legs and feet, and intense pains in the shoulders; but he did not meet with any cases in which chronic rheumatic pains occurred. With respect to the colour of the body of the fatal cases, they seemed in that respect like yellow fever, only there was no suppression of urine. He did not examine the intestines in any of the cases that died; but in one instance he did find that the mesenteric glands were not enlarged. With respect to treatment, in many instances the men objected to take quinine, thinking that it brought on sickness of the stomach; and the defervescence took place most rapidly in those that did not get it. He had found that large doses of quinine in cases of remittent fever had a paralysing effect on the liver.

DR. CHARLES A. CAMERON read a paper "On an Outbreak of Fever caused by Infected Milk."

On the motion of DR. LYONS, the discussion on Dr. Cameron's paper was adjourned to Wednesday, May 14. [This paper, with the discussion upon it, will appear in the next number of the Journal.—ED.]

APOMORPHIA IN CROUP.

A CASE corroborative of the value of apomorphia as an emetic in this disease is reported in the *N. Y. Med. Record* (April 26). Dr. James was called in great urgency to see a child of two years old, suffering from a rapidly-developed attack of croup. On arriving at the patient's house, the face was cyanosed and livid, the hands clenched, and from between the firmly-set teeth slowly exuded a frothy mucus. The struggle for breath, the gasping, whistling effort to inhale the air, was simply horrible, and it was plain that unless relief was very prompt the combat could not be maintained much longer. Death was near at hand. Dr. James at once injected the one-twentieth of a grain of hydrochlorate of apomorphia in solution under the skin of the arm. The child being held face downwards in his father's arms, the doctor, with watch in hand, awaited results. In two minutes and fifty seconds, almost without effort, copious vomiting came on and a membranous cast of the larynx and trachea was expelled. As if by magic, the terrifying and urgent symptoms disappeared, and the patient made a quick recovery.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

FORTY-FIRST ANNUAL SESSION.

EDWARD B. SINCLAIR, M.D., President.
WILLIAM ROE, M.D., Honorary Secretary.

Saturday, May 3, 1879.

E. B. SINCLAIR, M.D., President, in the Chair.

A Case of Transfusion. By DR. KIDD.

DR. KIDD read the following case, in which transfusion had been successfully performed, and said he was indebted for the notes to one of the students at the Coombe Hospital, Mr. Hickman, who had been in charge of the case:—

“Mary Cox, aged twenty-nine years, a strong, healthy woman; has had five children. Her four previous confinements were perfectly natural, nor was there ever any *post partum* hæmorrhage. During last winter she suffered from bronchitis, and in November was threatened with an abortion with severe hæmorrhage, and for three weeks previous to her confinement was deprived of any rest through having to nurse a sick child. Did not expect her confinement until the middle of May. Labour commenced on the 11th of April, a little before 4 a.m., and when I arrived, at 5 30, I found the os dilated. At 5 55 the membranes ruptured, and at 6 10 the child was born. The uterus contracted normally under my hand until about 6 30, when it began to relax, and immediately afterwards the blood came with a gush, which flooded the bed. I at once applied cold water, and lowered her head, and, on feeling for the uterus, could not distinguish its contour. The bleeding then ceased, and slight intermittent contractions returned. She became faint, her face extremely pale, lips blanched, and pulse scarcely perceptible at wrist. I administered stimulants to prevent syncope, and Mr. Connellan then found an hour-glass contraction. Dr. Mason, on arriving, ordered beef-tea and spirit, and gave two hypodermic injections of sulphuric ether, ʒi. in each, into gluteal region, and applied hot-water jars to feet, back, and abdomen. Dr. Kidd came about 8 30, when he expelled the placenta by external pressure, and at once determined to perform transfusion, the instrument being the one designed by Dr. M'Donnell. About twelve ounces of blood, which was taken from her husband, was injected into

right arm. This soon had a marked effect, bringing back the pulse to the wrist, a nutrient enema of beef-tea and a hypodermic injection of sulphuric ether being given immediately after. During the day Mr. Connellan continued this treatment, and at night all the hot-water jars were removed except the one at her feet.

"April 12th.—Slept well during night; was perfectly conscious. Several enemata were administered during day. Pulse 102.

"13th.—Slept well. Dr. Mason and Mr. Connellan syringed out uterus with Condyl's fluid and water; got a purgative enema; milk withdrawn from breasts. Diet, beef-tea and milk.

"14th, 15th, 16th.—Progress satisfactory; pulse and temperature remaining the same.

"17th.—Had slight rigors during night. Mr. Connellan syringed out uterus with Condyl's fluid and water, and gave enema. Great thirst; pulse 100; temperature 103°.

"18th.—Slept badly; complained of neuralgia; thirst and rigors gone; appetite good; pulse 100, temperature 100°.

"19th.—Pulse 96; temperature normal."

Those were the notes that Mr. Hickman gave him. When he (Dr. Kidd) saw the woman she was unconscious, quite pulseless, her surface was cold, and she was vomiting, tossing her arms about and calling for air. Fortunately Dr. Mason, who first saw her, sent a message to him to bring the transfusion apparatus, and he believed that his doing so was the means of saving the woman's life, for if he had not done so the delay that would probably have resulted would have occasioned her death. There was some little delay in obtaining the blood. Her husband was a nervous sort of man, and twice had the bandage taken off his arm, but the third time consented to be bled. He obtained from him about 12 oz. of blood, which, after it had been defibrinated, was injected into the woman's arm. When a small quantity was injected they thought the woman was gone. She heaved deep breaths, her eyes glistened, and it was evident that there was some difficulty in her respiration. On stopping the current for a moment those symptoms disappeared, and they then proceeded with the injection. At first there was no very apparent effect. Mr. Hickman in his note said that her pulse returned, but it was so feeble that it was doubtful whether it had or not. At the wrist they certainly could not feel it, but at the temporal artery they thought they did. Gradually, however, the heat of the body returned, and when he (Dr. Kidd) saw her in two hours afterwards she had a good pulse at the wrist, and a reaction had evidently set in. The operation was performed on Good Friday morning. When he visited her on Easter Sunday and spoke to her about the operation she was quite unaware that anything had been done. This was the sixth time that he had performed the operation of transfusion. At the first which he performed he believed Dr. Denham

was present; it was upon a lady residing in Leinster-road, a patient of Dr. Evory Kennedy, who was out of town, and in his absence he (Dr. Kidd) was sent for. It was a case of accidental hæmorrhage and premature labour. He found her in a state of extreme prostration—so great that he asked for assistance, and had the advantage of Dr. Denham's help; and having emptied the uterus, they determined to perform transfusion. While he was engaged in bleeding her husband, Dr. Denham said it was useless to proceed. To gain time he (Dr. Kidd) at once injected some warm water and salt, but it was too late—the patient expired while it was being done. His next operation was upon a patient of Dr. Churchill's, who lived in Camden-street. When he arrived with the apparatus, Dr. Churchill said he feared the case was hopeless. He (Dr. Kidd) injected ten or twelve ounces of blood, but the operation failed. He believed the reason of the failure to have been that the operation was delayed too long—in fact, they did not see the patient until all chance of recovery was gone. It was mentioned in the note of the present case that six days after the operation the woman had a slight rigor during the night, and her pulse rose to 101 and her temperature to 103°. That caused a good deal of uneasiness at first, and Mr. Connellan, the resident pupil of the hospital, at once washed out the uterus—though this had been previously done with Cond's fluid—thinking that there was some decomposing clot in it which was giving rise to septicæmia. It turned out, however, that the rigor had been caused by earache, the result of her head being close to an open window during the hæmorrhage. No ill results afterwards followed from the operation, and the woman was now up and attending to her household affairs. His impression was that the operation was quite devoid of danger, and that it was one which they should not hesitate to perform, even though they might think that the patient might possibly recover without it; and he also thought that it would be much better to perform it too soon than to put it off until it was too late.

DR. ROBERT M'DONNELL said he had not been present at the commencement of the communication, but he knew the particulars of the case, and that the woman was in a fair way of convalescence in a week after the operation was performed. He was extremely happy that another case had been added to the list of transfusions performed in this city with defibrinated blood. It was of great consequence that this case should be recorded, for, as he had stated at former meetings, a great deal of harm had been done as regarded the operation by observations occurring in an article in *The Clinical Lecturer*, in which the most terrible consequences were detailed as likely to follow from the use of defibrinated blood. They were told of bloody urine, of ecchymosis, of persons sinking with symptoms of hæmorrhagic smallpox, and of other hideous conse-

quences resulting from the use of defibrinated blood. The best answer to this was given by cases in which none of those dreadful consequences followed. In not one of the cases in which he had operated, and which proved successful, did any such result follow, nor, as they had seen, had they followed in the cases just now related by Dr. Kidd. When the patients died it was from the fatal hæmorrhage; when they recovered it was complete. They might, therefore, conclude that the operation was a safe one, and that if it succeeded at all it would do so without any of those ill consequences which had been attributed to the operation and to the use of defibrinated blood. He should have liked to make a few observations on a case in which he used milk for the purpose of intravenous injection, but as Dr. Meldon, who was present at the operation, intended to make a brief report upon it, he would merely allude to the case in the briefest way now. No doubt it would occur to some that milk might be used in some cases of *post partum* hæmorrhage, but he ventured to doubt that it would ever supply the place of blood for two reasons. In the first place, the milk should be fresh from the cow. Now, it would not be at all easy to get such milk on emergencies, especially in the houses of poor persons or in hospitals, whereas it would be comparatively easy to get fresh defibrinated blood. The freshness of the milk was a *sine quâ non*; the use of stale milk would, as they knew from experimental physiology, be exceedingly dangerous, and in the case of dogs the injection of it had proved fatal. There was another very grave reason why he did not think milk would answer nearly as well as defibrinated blood. In all the reported cases that he had seen of the intravenous injection of milk there had occurred subsequent periods of from half an hour to an hour or two hours' duration of considerable depression and great labour of respiration. That was no doubt the time when the milk was passing through the lungs, and the lungs were more or less blocked up by the milk globules. It was no doubt a period of struggling for existence, in which a patient who had been greatly depressed by profuse *post partum* hæmorrhage would sink. It was exactly the time when there was need of giving a fillip which would be something in the way of nutriment, and would revive the patient and enable her to tide over a condition of things in which she would otherwise be very apt to succumb. In every one of the cases of milk transfusion that he had read this great labour of breathing had been observed; but on the other hand, where there was a failure of strength and they desired to give nutriment, one of the most closely similar things to chyle that they could select was milk which contained a quantity of nutritive material that could be turned to account a little later on; but in a case of *post partum* hæmorrhage, and where they were not able to get blood, he thought that a saline solution was the next best thing to inject, because it should be recollected that the vessels were then in a state of comparative emptiness,

and one of the principal objects was to give the heart, which was still rapidly contracting and expanding, something to grasp—to supply it, in short, with a mechanical *point d'appui*. In cases of cholera, as well as of *post partum* hæmorrhage, the revival resulting from the injection of saline solutions had been found to be very considerable. Blood had, no doubt, the advantage of not only giving the heart something to act on, but of also supplying the red corpuscles which supplied the required stimulant in the greater number of cases. Dr. Roussel, who had devised an exceedingly ingenious but complicated apparatus for direct transfusion, disparaged the use of defibrinated blood in his work. But his objection was unfounded. They knew that the red corpuscles of defibrinated blood did not undergo any considerable change, and they also knew what was not less important—namely, that the white corpuscles not only continued to live, but they could see them with the microscope moving about in a saline solution. Therefore, the mere removal of the fibrin did not deprive the blood of the active influence of the red corpuscles, or of the animating influence of the white corpuscles. Therefore, it was not correct to say that blood deprived of its fibrin was nevertheless incapable of performing the important functions which all physiologists attributed to it in cases of the kind.

DR. HENRY KENNEDY said that when the last epidemic of cholera occurred in London a considerable number of cases were treated by transfusion of saline ingredients. The quantity of fluid used in those cases struck him as very remarkable; and some of the persons recovered after they were absolutely pulseless, blue, and voiceless. The quantity of saline fluid injected was in some instances so much as eighty ounces. Such a quantity of defibrinated blood could not be obtained in the case of females suffering from loss of blood, yet he did not see why cases of the kind might not occur in which such a quantity of blood might be requisite. Therefore the time was probably approaching when some other fluid besides blood would have to be used in such cases. He agreed with Dr. M'Donnell that milk was not a fluid which could be used with safety under such circumstances. The difference between even the purest and freshest milk and chyle was too great. He saw a dog bled from the jugular vein to such an extent that the animal lay apparently dead; a quantity of tepid water was then injected into its veins, and to the utter astonishment of all who witnessed the operation the dog got up and staggered about, although it died after some hours. This showed the effect of merely supplying the heart with something to act on independent of mere nutriment.

DR. MORE MADDEN thought the case Dr. Kidd had brought forward encouraged them to practise this most useful operation; at the same time he did not believe that the modes of performing it had as yet arrived at perfection. In order to be used generally it should be practicable.

It might be theoretically and physiologically perfect and yet be wanting in the great merit of being easily performed in emergencies. It was generally required when a woman was at the last gasp from flooding; and he feared it would be impracticable in remote districts, where the aid of gentlemen so skilled in the performance of it as Dr. Robert M'Donnell could not be had. It would therefore, he believed, tend to discredit the operation if they made the performance of it dependent upon exceptional surgical skill, exceptionally efficient assistants, and complicated apparatus. For his own part, if he were obliged to resort to transfusion, under circumstances where exceptional skill was not to be had, he would rather trust to a saline infusion than attempt to obtain and use blood. He therefore thought that perfection in the operation of transfusion was as yet *in futuro*.

DR. MACAN said Dr. Kidd had stated that the quantity of blood he used was twelve ounces; but much less quantities had been found to produce most magical results. The saline injection produced merely mechanical effects; and so far as the mechanical effect of six ounces of blood went, it was as nothing to make up for a loss of forty ounces. The stimulating quality of blood was in fact one thing, its mechanical effect dependent on the quantity injected was another. According to his reading of the cases reported, the most successful of them had been cases in which from four to eight ounces of blood had been injected to replace much larger quantities of blood lost. According to Volkmann, the Germans employed transfusion to produce contraction of the uterus, and they certainly preferred blood which had not been defibrinated. The difficulties and dangers of the operation seemed entirely wiped out if they used defibrinated blood. With the exception of the danger of the entry of air into the veins, the main danger was the coagulation of the blood between the time of its leaving one vein and entering the other. But if they used defibrinated blood the danger of coagulation was done away with. In spite of Dr. Kidd's case he was not an entire believer in transfusion, but in a great many instances it would give the necessary stimulus that the heart wanted.

DR. GEORGE JOHNSTON said he witnessed one case of transfusion in which not two ounces of blood had been injected before the pulse began to be felt, and after the injection of from 6 to 8 ounces, the patient recovered without any bad symptom whatever.

DR. M'CLINTOCK said a great advantage to science would result from the reporting of cases of this operation. Dr. Kidd's case was a very instructive one in his mind, and showed that this operation was capable of saving life under circumstances when life seemed to be almost gone.

DR. KIDD, in reply, said the suggestion of Dr. Kennedy as to the use of a saline solution was very valuable. He had already mentioned a case in which he had injected a saline solution. He did not see why they

should not combine the advantages of quantity and quality by first injecting some 8 or 10 ounces of defibrinated blood, and following that up with the injection of a saline solution; or, if the blood were not ready, the saline solution might be injected first. The advantage of the saline solution was that it could be made of such a density as not to cause the rupture and deliquescence of the red corpuscles. No doubt, under some circumstances, the saline solution could be made of a stimulating character in itself. Many years ago papers were written by Dr. Halford on the advantage of injecting ammonia in cases of snake-bite for the purpose of stimulating the heart's action. He (Dr. Kidd) tried it in a case of hæmorrhage, by adding some ammonia to the defibrinated blood, with an extern patient of the Coombe Hospital, but it did not succeed. He repeated that in transfusion the great thing was to perform the operation promptly—it was better to be too soon than too late. As to the alleged difficulty of the operation, he did not think any man was fit to be a surgeon who would not be able to perform it without having ever seen it done. The only difficulty was to find the vein in the arm of the bloodless woman; and after it was found, a probe should be placed under the vein before it was opened. Dr. M'Donnell's apparatus was a very simple and cheap one. If they adopted a direct transfusion of blood from one arm to another, as Dr. Aveling suggested, it would, no doubt, be speedier than the other mode; but there were some objections to it. First, there was the danger of embolism arising from the coagulation of the blood in passing through the India-rubber tube from one arm to the other. In the next place, if the husband were bled in the presence of his wife—lying almost at death's door, tossing about, crying for air—the supply of blood that was likely to be obtained from him under such depressing influences was likely to be very deficient. Dr. Macan had referred to a German practice of intravenous injection of blood in its natural condition as a means of causing the uterus to contract; but he believed all opinions in this country were in favour of arresting the hæmorrhage before transfusion of any kind was attempted. It did not seem to be of much use to inject a few ounces of blood into a patient's vein while double that quantity was going out of her uterus.

Case of Acute Inversion of the Uterus. By DR. DARBY.

MRS. O., a healthy woman, thirty years of age, who had been nine years married, and had arrived, as she averred, at full period of her eighth pregnancy, when stooping suddenly to pick something off the carpet, on evening of 26th July, 1878, felt a great gush of blood escaping from the vagina. She lay down and sent for the nursetender. The flooding continued all night without any pain or symptom of labour. The next morning, while the bowels were acting, a large quantity of blood was passed; and, although she said she felt quite well, her husband, who did

not consider it right to allow this state of things to go on, sent for me. I was at her bed side at eight o'clock a.m. She looked well, with red lips and good complexion; her pulse 80, full and steady, and her feet warm; but her night dress and sheets were saturated with blood, and there were several large clots in the bed. I removed a large clot from the vagina. The os was quite closed, but soft enough to allow the tip of my finger to pass through it without resistance, when the placenta was felt completely covering it. The abdomen was so soft and relaxed that I felt the outline of the child distinctly, but could not feel the uterus. I plugged the vagina tightly, and placed the binder lightly round the abdomen. I then left the case in charge of Dr. Alexander Leney, with directions to send for me, should symptoms of labour come on. I was sent for in an hour and a half, and on my arrival found that blood was escaping freely beside the tampon. Mrs. O. was pale, cold, and excited, with a very quick, thready pulse, but no pain. On removing the plug, I found the os in exactly the same state as at the previous examination. I at once proceeded to turn, and experienced no difficulty in introducing the fingers of my right hand through the os. I passed a stilette through the placenta, ruptured the membranes, and then felt what I took to be an arm; but, as my hand was peremptorily arrested at the knuckles by the os, which was like a band of iron, I had not much power, but was obliged to manœuvre with my fingers as best I could. After a little, assisted by my left hand on the abdomen, which was still soft and relaxed, by rolling the child from side to side, I at length succeeded in catching the foot between my fingers; this, by gentle traction, materially aided by my left hand, I succeeded in coaxing into the vagina. At this period I administered ʒij. liq. ergotæ. The delivery was then accomplished without further difficulty. While I was occupied say for ten or twelve minutes endeavouring to resuscitate the child, which was barely alive, Dr. Leney maintained pressure over the uterus, and the placenta came slowly but spontaneously away without further loss of blood. At that moment Mrs. O. cried out "Oh! I have a dreadful pain; it is not like a labour pain—it is sharp, like a knife through my left side; I cannot see you; the sea is roaring in my ears; my God! I am dying." She was bedewed with cold, clammy sweat, and the pulse had forsaken the wrist; she looked like death, and I thought she was dying. Leaving the child to its fate, and placing my right hand over the hypogastrium, which I felt hollow and empty, I introduced my right hand into the vagina, and found it filled with a mass resembling a placenta both in size and shape, and I feared I had to deal with a case of ruptured uterus. On moving my fingers round this mass, however, in search of a laceration, I could not find one; and then, maintaining with my whole right hand a sort of grasping upward pressure, I had the satisfaction to feel the mass receding, while with my left hand I perceived it rising into the epigastrium. My right hand was now

completely within the cavity of the uterus, where I allowed it to remain for a minute or two, and stirred my fingers, in the hope of inducing some contraction of the organ, but without producing that much-to-be-wished-for effect. Mrs. O. now said "The pain is gone, and I feel better." Reaction set in, the pulse returned to the wrist, and warmth, slowly spread over the body and limbs. Mrs. O. recovered without any untoward symptom. In this case, you observe, I had to deal with inertia uteri, placenta prævia, an alarming amount of hæmorrhage, and what I believe to have been acute inversion. And, although I congratulate myself on the recovery of my patient, and may say "there is nothing so successful as success," I am by no means satisfied that in each phase of the case *the best possible* means for saving life were adopted; and I am, therefore, anxious to hear it freely discussed by those now present, whose skill and experience far transcend my own. I cannot conclude without expressing my thanks to Dr. A. Leney for his promptness in coming to my assistance, and for the kind and unremitting attention he paid throughout the after-progress of the case.

DR. M'CLINTOCK said that cases such as Dr. Darby had brought forward were extremely rare. In his experience he had never met with a case of acute inversion of the uterus under circumstances such as Dr. Darby had described. The nearest thing he had seen to it was a case that occurred a few years ago, in which, on the tenth or thirteenth day after delivery, an inversion of the uterus occurred. Dr. Churchill and he put the woman under chloroform, and after some careful manipulation succeeded in turning the vagina round with the hand, and the woman recovered. During his (Dr. M'Clintock's) mastership of the Rotunda Hospital he might mention that a woman was under his care for a miscarriage and afterwards died of puerperal fever. While she was in the hospital she mentioned to the nurse that she had been previously in the hospital, and had suffered from a serious accident—namely, an inversion of the uterus. He (Dr. M'Clintock) made inquiries from her, and learned that she was subjected to a good deal of violent pulling and put to great pain, so that he was inclined to think there had been an exercise of *nimia diligentia* in that case. At the same time he believed that the uterus might be inverted without any improper interference whatever on the part of the accoucheur. It was well to have that clearly understood, as otherwise cases might occur in which an enormous weight of unjust censure would be thrown upon either the nurse or the practitioner.

DR. MORE MADDEN said a case came under his notice which unfortunately did not end so well as that recorded by Dr. Darby. Some years ago he was sent for to see a woman on Ellis's-quay. She had been half an hour previously attended by a midwife, and immediately after the expulsion of the child flooding commenced, and the nurse becoming

alarmed, sent for Dr. Torney, who sent for him. He found the uterus completely protruded, and also discovered that the placenta was adherent. Stimulants were administered, the placenta removed, and the uterus restored, but the woman sank, and died from excessive loss of blood. The nurse assured them that she had not either exerted any traction of the cord, or made any pressure over the fundus. He saw another case of a woman who had an inversion of the uterus for three months. She had been attended by a most excellent midwifery practitioner, who assured him that he had neither pulled the cord nor pressed unduly upon the fundus.

DR. KIDD said he did not think any fact in midwifery or physiology was better established than that inversion of the uterus might occur spontaneously from segmental contraction of its fibres, and it was an accident that might occur without any malpractice.

DR. FITZPATRICK mentioned a case of a lady he was called in to see. She had been attended in her second confinement by an eminent practitioner. She told him that her womb was down between her legs. On examination he found that a portion of the membranes had been left, and was causing hæmorrhage, being about the size of a cricket-ball. He cut it away, and relieved her in a few minutes.

DR. KIDD said that in illustration of the phenomenon of segmental contraction of the uterus after delivery, he might mention a fact which, when he first became acquainted with it, startled him a good deal. He was more familiar with it now, and believed that it was universally present. If after delivery the finger be introduced as far as the os, there would be found a remarkably hard, prominent, convex swelling, most easily and commonly found in the anterior wall of the uterus, but also to be found at times in the posterior, but higher up and more difficult of access. This was not mentioned in books, and when he first observed it, it occasioned him a great deal of alarm. He believed now, however, that this was always present, and that it was simply the result of this segmental contraction of the uterine fibres which formed a large, hard, round swelling.

DR. M'CLINTOCK described a similar hard swelling which occurred in the case of a lady whom he was called on to see. It was in front of the os, and at the extreme edge. She had suffered from some anomalous febrile attacks during pregnancy, and from hæmorrhage at delivery, but recovered perfectly.

DR. DARBY, in reply, said he had at first some doubts as to the correctness of the treatment he had adopted in the case he had brought before the Society, but he was glad it had been approved of by such men as Dr. Kidd and Dr. M'Clintock.

The Society then adjourned.

TRANSACTIONS OF THE ULSTER MEDICAL SOCIETY.

SESSION 1878-79.

President—A. HARKIN, M.D., J.P.

Hon. Secretary—WILLIAM WHITLA, M.D.

April, 1879.

A. HARKIN, M.D., J.P., President, in the Chair.

The Removal of a Fibrous Tumour from the posterior wall of the Uterus; and a few Remarks on some other cases of Uterine Tumours. By PROFESSOR DILL.

MR. PRESIDENT AND GENTLEMEN,—Had I reflected for a moment when I at first proposed to bring this case under the notice of the Society, I should have also stated that I would use the opportunity to offer a few remarks on some other cases of fibroid tumours; and, with your permission, Sir, I may do so still. I have not had sufficient time to bring this subject before you in such a way as I should like, or as its importance deserves; but I think it is possessed of sufficient intrinsic interest to make it acceptable.

The subject of the case to which I would first beg to direct your attention is that of an unmarried woman, about thirty-five years of age. On the 10th July, 1878, I was asked to see her in consultation with Dr. Wilson, of Whiteabbey. She had been complaining for seven years, and her illness was principally recognised by the fact that she suffered much from frequent attacks of menorrhagia. At the time of my visit there was a profuse discharge of blood. She suffered much from dysuria, irritation of the rectum, and irregularity of the bowels; she had lost flesh considerably, and was very anæmic. On making a vaginal examination, I discovered a large tumour which engaged nearly the whole of the pelvic brim, the base of which appeared to be embedded in the substance of the wall of the uterus, and extending from a little above the posterior lip of the os to a point very near to the fundus uteri. That part of the tumour which presented was smooth, somewhat globular, very firm, and like a child's head offering at the brim of the pelvis.

Although the patient very much desired it, yet we did not think, from the state she was then in, that we would be justified in operative interference. She was placed upon iron, ergot, sedatives, with a full diet, under which treatment the symptoms, general condition, and her strength

became improved after some time. No alteration, however, appeared to take place in the tumour.

On the 18th March, 1879, I was again asked to see her with Dr. Wilson. The tumour had now appeared to have increased a little in size from the time I had first examined it; and, as the patient had become anxious about her state, and desired, if possible, that the tumour should be removed, and as we could not discover any insurmountable difficulty in the way, we consented to enter at once upon the operation for its removal. We placed her upon the left side, near the edge of the bed, or just as in the case for an ordinary confinement. As she refused taking chloroform, I gave her a 2-grain pill of opium, which I have found useful in protracted operations either with or without chloroform. The bladder and bowels having been previously emptied, the first step in the operation was to get the tumour down and within easy reach, which was attempted by seizing hold of it by means of a vulsellum. This instrument was not found of sufficient strength or power, neither did it keep a good hold of the tumour, and had to give way to the midwifery crotchet with the shield, which answered the purpose uncommonly well, as by this instrument the mass of tumour, which yielded slowly but surely, was safely brought down and outside the vulva. The base, or its attachment to the uterus, which was broad, was then transfixed with a large needle, armed with a double ligature of whipcord. The ligature, being brought half way through, was divided and tied firmly on each side. Although the operation was a tedious one—lasting altogether more than two hours—yet the patient did not appear to suffer much when it was over, and she slept well the first night, which I ascribe to the timely use of the opiate. The ligatures came away at the usual time. Since that she has menstruated naturally, and she is now—two months since the operation—going about her ordinary business, strong and well. The tumour weighed 9 ozs. 7½ drs. I have given small portions of the tumour which I now exhibit to Dr. Workman, who is prepared to give you his microscopic observations of it. I may conclude this case by stating that, from the first, I was satisfied of it being a fibrous tumour and not at all malignant.

The second case is one which a few words will dispose of. The patient, after being married for five years without any issue, got into bad health, with occasional attacks of menorrhagia. A very large pelvic tumour had existed for a long time. I then saw her with Dr. D. Johnston, when the tumour was so large that it could not pass the brim of the pelvis. We concluded, from its size and from other reasons, that it was not a case for operative interference, and prescribed iron, ergot, &c. We saw her again, in consultation, some short time afterwards, when we thought the tumour had somewhat diminished in size. We prescribed some local application and bromide of potassium inwardly. She was sent to the country, and, again, to the seaside, for change of air. I saw her a third

time, at the end of a year, when I found that the tumour had completely disappeared. I could get no exact history as to how it decayed or disappeared, but that she had become quite well and free from the tumour was without doubt.

The third case is one very similar to the second—a patient of about forty years of age. She had been the subject of many attacks of excessive menorrhagia. I saw her on one of those occasions with Dr. M'Crea, when we prescribed for her, and suggested that at some future time she might be fit to undergo an operation. I saw her, back and forward, for some weeks. She was then sent to the country, and when she returned, at the end of twelve months, the tumour was quite gone, and she appeared to be in tolerably good health.

The fourth case I saw with Dr. H. M. Johnston, who sought my assistance in a complication of labour by a large fibrous tumour. I examined and found a very large, hard tumour of the uterus, low and to the left side. The head of the child could neither be induced in any way to pass the tumour nor could the tumour be induced to pass up beyond the head. After many fruitless efforts to relieve the woman, and being satisfied that now the child was dead, we resolved on reducing its head, and we experienced no difficulty in extracting it. The tumour was such as to invite immediate removal, which was done, and the mother made a good recovery.

The older members of the Society will remember (as I well do) a very interesting case introduced here by the late Dr. Johnston, which occurred in his practice in the Union Workhouse. It was that of a poor woman who took ill in labour in that institution. Dr. Johnston had watched the case with interest for some time. The os was dilating, but the head was making little or no progress. He left her for a short time, and during his absence the pains, which had been strong, suddenly ceased, and, before he could get back to her, the woman was dead. A *post mortem* examination was made, when a large rupture in the body of the uterus was discovered. A fibrous tumour the size of a cricket ball was found growing at the fundus uteri, towards the right side, and it was also firmly attached to the lower edge of the liver. Evidently the tumour had risen with the increasing size of the pregnant uterus, and in the end it had been pressed against the liver, to which it had become adherent, thereby so bridling the uterus as to prevent it from contracting, descending, and expelling the foetus. The result was rupture of the uterus, and, consequently, the death of the mother and the child. Both might have been saved if the woman's condition could have been diagnosed or clearly anticipated.

My sixth and last case was that of a lady whom I attended for a long time. At first she suffered from severe pain in the lower part of the abdomen and towards the left side. A tumour could soon be discovered in the same region. I was always satisfied, from its position, shape, and

size, that it was attached to the uterus, and that it was of a fibroid character. This lady consulted many surgeons and physicians, both here and elsewhere, but it was not satisfactorily diagnosed by any of them, nor the exact nature of the enlargement stated. At the end of more than three years of suffering and doubts regarding the result of the disease, suddenly she was seized with very acute pain and torture, which after a few days became somewhat relieved. I could now feel the tumour freer and rather movable in the abdominal cavity. After some weeks I thought I could feel it becoming fixed, and in the end adhering to the inner and peritoneal surface of the abdominal walls. The abdominal parietes became inflamed at their point of attachment, and after being poulticed for a time, an abscess formed, gave way, and discharged for a long time. The lady eventually recovered, and she now enjoys good health.

The fifth and sixth cases are full of interest and valuable instruction, because of their varied condition and the somewhat peculiar termination of each. They also verify the fact that such tumours are not confined to the mucous surface, but that they also form upon the peritoneal side of the uterus, and that fibroids may form upon or take as their origin any point of uterine fibre.

These cases may, I trust, though imperfectly stated, be of some interest, and form a small contribution to your "Transactions." The first is of interest in the fact of the woman having been so long ill and in making such a complete recovery after the operation. The second and third cases are possessed of interest from having made—shall I call it?—spontaneous recoveries, by involution, atrophy, and decay. The fourth was one of unusual interest, complicated as it was with pregnancy and labour, and, with all, the woman having made a satisfactory recovery.

ULCERATIVE PHTHISICAL LARYNGITIS.

DR. BEVERLEY ROBINSON has an interesting communication in the April number of the *American Journal of Medical Science* on the nature of the above affection and the value of tracheotomy in its treatment. He believes that tracheotomy, at an early period of chronic disease of the larynx, looked at in regard to its indications, and its possible or probable results, merits attention as a *prophylactic*, or as a *remedial* operation. Personally, he is convinced that tracheotomy will be frequently performed, within a brief period from the present, as a trustworthy *remedial* operation in ulcerative phthisical laryngitis—1st, because he believes that ulcerative phthisical laryngitis is usually non-tubercular in its nature, and, therefore, curable. 2nd, because tracheotomy seems to be the best, if not the sole, means of directly attaining the end.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

President—WILLIAM MOORE, M.D.

Secretary—E. H. BENNETT, M.D.

Necrosis of the Tibia.—MR. THEODORE STACK said: This specimen was sent up to me by Dr. Thompson, of the Tyrone Infirmary, with the following note relating to it. The specimen was not sent up immediately after amputation. The death occurred during the severe frost, and the corpse was buried and afterwards taken up, and the limb removed, in order to be shown here. The limb at present shows a stage of ulceration of the skin later than existed when the patient was admitted. The absence of the tibia is one point of interest, and another is the ligature of the femoral vein.

History.—James Gray, aged thirty-four, a very anæmic, unhealthy-looking man, was admitted into the Tyrone Infirmary, suffering from extensive disease of the tibia of the left leg. He stated that about eighteen months ago he received a sprain when ploughing, to which he paid but little attention. In a few days his leg began to swell, and matter formed and burst. Several pieces of bone were discharged, and there was constantly a profuse discharge of yellow foetid pus. The knee gradually contracted, and his health grew worse daily. No surgeon was consulted and no treatment was attempted previous to his admission to the Tyrone Infirmary.

Symptoms.—Great anæmia and wasting of the body generally. The face had a yellow pasty look, and the limbs were hardly the thickness of a child's. The left leg was bent at a right angle, and rested on the middle of the right leg. It was slightly swelled, and, from immediately below the tuberosity of the tibia down to the epiphysis, close to the ankle-joint, the tibia was entirely absent. There was a profuse discharge of unhealthy pus. The skin of the leg was extensively ulcerated, and seemed much diseased, extending underneath the knee-joint, and for a short distance along the outer side of thigh. The knee appeared ankylosed, as did also the hip. The urine was albuminous, and under the microscope the white corpuscles of the blood were greatly increased in number—the red being much decreased. There was no unfavourable family history, and the patient's health was good previous to the accident, which he blamed for his present condition.

Treatment.—Evidently nothing capable of affording a shadow of chance for life except amputation. This operation was accordingly performed,

about the centre of the thigh, by an ante- and post-skin flap, at the urgent request of the patient. Several arteries required ligature. So difficult to control was the venous hæmorrhage—pressure and elevation of the limb, &c., having no effect—that I was compelled to ligature the femoral vein. Whether or not this was followed by inflammation I cannot positively state; it is very probable, from the rapid elevation of temperature, which went up continuously after the operation to 103° on the third day, that it was. From this time the patient rapidly sank, dying the next evening comatose.”—*January 18, 1879.*

Fracture of the Scapula.—DR. T. E. LITTLE said: I am desirous of exhibiting the present specimen as it is one which illustrates some peculiarities of the pathological anatomy of fractures of a bone which—as compared with the other bones of the body—is comparatively rarely broken.

The bone was removed from the body of a dissecting-room subject; and I am, therefore, unable to give any clinical details or history of the case. One point, however, of intrinsic evidence with regard to these particulars we may gather from the fact that this scapula was obtained from the same body which has afforded the specimen of fracture of the femur presently to be shown by Professor Bennett. From the coexistence of these fractures, which are presumably of the same date, we may, I think, fairly conclude that the accident causing them was one of exceptional severity.

The body was that of a rather stout and muscular man, of middle age. The bone belongs to the right side—the opposite to that on which the fracture of the femur had occurred.

As to the fracture of the *body* of the scapula, it has the position and direction which appear to be most common in such cases—i. e., it is transverse, traversing the infraspinous fossa just below the spine. The displacement of the fragments will be seen to be singularly slight, and the osseous union firm and good. As to the actual displacements which have occurred, the lower fragment is drawn upwards and backwards; it is drawn slightly outwards; it is rotated outwards in such a manner that, by the advance of the inferior angle, the external border of the scapula is somewhat shortened, and the internal border (or base) elongated; and it is rotated backwards so as to obliterate the normal concavity of the venter, and present an angle slightly salient in front. Towards the base of the scapula, along the line of union, we observe several more or less oval-shaped perforations of the bone, evidently due to absorption of the osseous tissue in the neighbourhood of the fracture. The fracture of the *coracoid process* is placed beyond the attachment of the coraco-clavicular ligaments. The line of fracture is oblique, from above, downwards, forwards, and outwards. There is no attempt at

osseous union, the broken fragment being connected to the base of the process by a very strong band of fibrous tissue, uniting the broken surfaces, and nearly half-an-inch in length.

The structures of, and related to, the shoulder-joint were, with the exception of the fractured coracoid, quite normal and healthy. It is evident that the humerus had not at any time been dislocated from the glenoid cavity.

Remarks.—The occurrence of the double fracture of the scapula itself, and of the fracture of the femur along with it, are facts confirmatory of the received opinion as to the severity of the accident necessary to cause the breaking of the bone in question, and more especially of its coracoid process. The fact that the fracture of this process occurs in the great majority of cases in which it is met with (at least in civil practice), as a complication of dislocation of the humerus, it is which has led me to notice the integrity, in the present case, of the structures of the shoulder-joint.

That fractures of the body of the scapula will readily and accurately unite by bone, while those of its processes seldom do so, are facts of very old observation, and the observation is illustrated in this case. I would wish, however, in reference to the union of old-standing fractures of the scapula, to call attention to one point more—I mean the existence of these curious roundish holes in the bone along the line of fracture which we observe in the present specimen, and which form so singularly constant a feature in all cases of the kind. I find them to be present in almost every specimen which I have had an opportunity of examining, or have found described or depicted. They will be observed in these specimens [exhibited] from the Pathological Museum of Trinity College. The appearance of the perforations referred to, and their uniformity of occurrence, show them to be plainly due to absorption of the osseous tissue, as the result of the fracture. An acquaintance with their frequency of occurrence as a feature in the pathological anatomy of old fractures of the scapula is the more important, as the appearances presented might otherwise be mistaken for the result of disease—a mistake which I know to have been, in one case, actually made.—
January 25, 1879.

TRANSACTIONS OF THE CORK MEDICO-CHIRURGICAL SOCIETY.

SESSION 1878-9.

President—DR. D. D. DONOVAN.

Secretary—DR. D. C. O'CONNOR, Jun.

Report of a Case of Concurrent Diphtheria and Scarlatina.

By S. O'SULLIVAN, M.D.

MR. PRESIDENT AND GENTLEMEN,—I beg to bring under the notice of the Society the following case, which came under my observation, and which I saw in consultation with Dr. Hobart :—

A. B., a boy aged thirteen months, was brought to me on the 4th January last by his mother. She stated that she noticed him ill and feverish on the previous day (3rd). On examination, I found the child anæmic; very fretful; tongue red, with what appeared to me to be aphthous spots on it; cutting back teeth. Observed a hard swelling—the first stage of a diffuse cellular inflammation—in right axillary and pectoral regions; bowels confined; abdomen slightly distended; suffers from restlessness and want of sleep.

5th.—Child brought to me to-day. He has a slight scarlatinal rash covering his face and neck; tongue red; aphthæ somewhat less; axillary swelling increased, with indications of pointing at its centre; bowels moved. Directed that the child should not be again brought out of the house; and, if not better, that I should be sent for on the following day.

6th.—Child was reported much better by the father, and I did not see him again until the—

7th, when I was requested to visit him at his own house. The rash has disappeared; ashy-gray coloured patches have appeared on the soft palate, tongue, inside of cheek, gums, and pillars of the pharynx; not observable on the back of pharynx or tonsils, which are inflamed, the tonsils being enlarged. The submaxillary glands are but slightly swollen; pulse rapid and weak; temperature 101°. There is much prostration. The bowels are constipated, with tympanitic abdominal distension; the pectoro-axillary inflammation redder, and apparently pointing at the lower margin of the pectoral muscle. There is an oedematous swelling extending from the shoulder to the hand on the same side. There is much pain in the axillary region, which is relieved by keeping the arm out from the side. No albumen in the urine.

8th.—General condition much as yesterday. Made an exploratory puncture with a long, narrow bistoury into the "abscess," at what appeared the point of softening, under the antiseptic spray. No pus was discharged, but the opening gave vent to a slight discharge of sero-sanguineous fluid. The part was dressed antiseptically (Lister's plan).

9th.—Condition of mouth and palate improved. Takes nourishment well. Pulse and other symptoms as before.

10th.—On removing the antiseptic dressing the puncture is found nearly healed. Swelling still hard, with the exception of the central part, which feels soft and is brawny looking, presenting the appearances observed in *post-scarlatinal* cervical abscesses. Diphtheritic patches still persistent, and rather increased. Angles of the mouth ulcerated. Breathing, for the first time, observed to be dry and croupous. There is great abdominal distension, with constipation and pain, which cause much restlessness and want of sleep. On this day and the 11th some of the diphtheritic patches peeled off, and the surface beneath presented an ulcerated appearance. During the night of the 10th the breathing continued very croupous; he had several attacks of laryngeal spasm, the head being thrown violently backwards, as if to catch air. These spasms caused much anxiety to the parents who, during the child's illness, alternately attended him day and night. The œdema of the hand, forearm, and arm increased; abscess larger, but still hard. The puncture made on the 8th has re-opened, and a slight sero-purulent discharge continues to come away on the poultices which have been applied. Skin of the neck desquamating. From this day the bad symptoms have been gradually lessening up to the—

15th, when a general improvement in the condition of the patient is observed to have taken place. The diphtheritic patches have disappeared. In one or two places, especially on the under-surface of the tongue, observed a highly granulating ulcerated surface where the pellicle had been. Around the anus, and in the gluteal region, the skin is desquamating. Swelling in the arm and forearm less. The abscess is discharging freely. As the opening is rather high, a counter-opening is made lower down, in the most depending part, and a drainage-tube inserted.

17th and 18th.—There appeared, commencing near the upper eyelids and extending over the face and neck, an eruption resembling measles in its crescentic shape, but larger, and more like erythema. The family being subject to eruptions of "hives," we did not attach any importance to this eruption, which disappeared on the following day.

20th.—General condition much improved. The abscess still discharging. Œdema has entirely disappeared from arm and forearm. The child is much emaciated.

1st February.—Up to this day the improvement has been steady and

continuous. A small circumscribed abscess, which appeared on the neck some days since, opened spontaneously.

5th.—On the right thigh a circumscribed abscess, about $\frac{3}{4}$ of an inch in diameter, which had been forming for some days, and which had a dusky colour, was opened, giving vent to pus mixed with a little dark blood. This was the last in a train of symptoms in this case which, as it appears to me, indicated the presence of the diphtheritic and scarlatinal poison affecting the one individual simultaneously. It would be an interesting problem to solve how far the one disease modified or aggravated the other; and in what manner the child became affected? The child is now perfectly restored to health.

That the rash was scarlatinal I have no doubt from the fact that it was succeeded by desquamation, and that it appeared at the usual time in the course of the disease—about the second day. I am also satisfied that the appearances observed about the mouth and throat were diphtheritic, as they were followed by marked laryngeal symptoms, and by the general infection of the system. The appearance of the patches, with the subsequent ulcerated condition observed in the mucous membrane on their disappearance, I believe prove their nature.

The family residence of the child is situated in a healthy locality, on elevated ground, outside the borough. The house is detached and isolated; there is a fruit and vegetable garden surrounding it, around which there are rather high walls. There is no system of drainage—all the excreta, &c., being thrown into an ash-pit some distance from the house, then covered with earth and ashes, and finally utilised on the surrounding soil. Some days after my first visit, I ascertained the existence of manure-heaps in a field on the opposite side of the road, the smell from which was exceedingly offensive. I have reason to believe that the drinking-water in the neighbourhood is liable to pollution. So far as I could ascertain, the child was not exposed to any other source of contagion, either of scarlet fever or of diphtheria.

Within the past fortnight (26th March) I attended two patients. One was affected with spongy gums, an ulcerated condition of the palate, enlargement of the cervical glands and tonsils, with high fever. This case—a young man about eighteen years—lived in the flat of the city, in an unhealthy locality. He made a good recovery in about a week. The other was a gentleman living in an elevated and healthy situation. He suffered from high fever, with sore throat. The right tonsil was much enlarged, and there appeared on it an ashy-gray pellicle, which I could peel partially off around its edges, but which was firmly adherent in the centre. The membrane disappeared in a few days, and the fever with it; but he then got an attack of ordinary sore throat, to which he is very subject. In a detached villa near this gentleman's residence I ascertained that there was a lady recovering from a

protracted attack of diphtheria. I mention these cases to show the possible sources of infection existing in the city. I will conclude this case with a history of the treatment adopted, and, with your permission, I will reserve any further remarks on this most interesting subject for a future occasion. I may observe, however, as an important point bearing on the history of the case, that there are six adults and two other children living in the same house, and that none of them up to this time (26th March) have been attacked. The father and one child did suffer for one or two days from a slight sore throat, to which the father is liable, and which did not require any treatment.

Up to the time when I first saw the child the mother was nursing him. This I directed her to discontinue, but, nevertheless, she did nurse the baby for some days subsequently. Notwithstanding repeated warnings, I could not prevail on the mother to give up kissing the child. Throughout the treatment the child got milk, or milk and lime water, chicken-broth, or beef-tea, and wine or brandy—the latter in small quantities in proportion to his age. He was kept in a room the temperature of which was maintained at 60° to 65° Fahr. Steam was constantly diffused through the apartment, and the carbolic acid spray directed to be freely used about the place, especially in the neighbourhood of the child. As a local application the liquor ferri perchloridi ℥iii. to glycerine ℥v. was freely applied to throat and mouth, and subsequently any ulcerated spots left after the throwing off of membrane were lightly touched with a solution of nitrate of silver. To the abscess on the breast the extract of belladonna was applied once or twice, but subsequently a linseed-meal poultice was used. I think it possible that pus would have formed sooner in the abscess only for the applications of the belladonna. The antiseptic treatment of the abscess used at first, when opening it, was given up also in favour of the poultice, and I think properly so, if we arrive at the conclusion that the septic matter from within was more to be feared in this case than the entrance of septic organisms from without, and that the antiseptic application would have a greater tendency to imprison the major evil within than to exclude the minor evil outside.

When seen on the first day, he got a purgative of calomel and rhubarb, and subsequently a dose or two of castor-oil. These medicines had to be subsequently supplemented by large enemata of turpentine, assafoetida, and caraway decoction, in consequence of the continued tympanitic distension, with constipation and abdominal pain, from which he suffered. A mixture containing chlorate of potash, with acetate of ammonia, was first ordered on the appearance of the rash. This was changed for a mixture containing tinctura ferri muriatis, ℥i.; potassæ chloratis, gr. xii.; acid hydrochloric dilut., ℥x.; syrupi, ℥iv.; and aquæ ad, ℥iv., ℥i. every fourth hour. When convalescence commenced,

this mixture was changed for another, containing muriate of quinine and Parrish's syrup, which was given with cod-liver oil. The laryngeal symptoms were combated by small doses of Dover's powder (gr. $\frac{1}{4}$ every fourth hour), and the attendant was directed to bend the head forward when the spasm came on. The Dover's powder appeared to produce a most salutary effect in relieving the spasm and in calming the child, quieting restlessness, and bringing on sleep. A hot linseed-meal poultice was kept constantly around the throat, the soothing effect of which was manifest. This treatment, as well as the breathing of the warm vapour in the room, was attended with the happiest results. The soothing effect of the latter could be noticed, for when the steam was allowed to go down, the throat symptoms and croupous breathing increased, and were again speedily lessened when the vapour was restored.

ERGOT IN THE TREATMENT OF PNEUMONIA.

DR. J. T. WELLS, referring to the treatment of this disease in the Bellevue Hospital, N. Y.—an account of which was published in the last number of this Journal (p. 469)—states that in his practice ergot has yielded better results than any other treatment. In all of the ten cases in which he employed this drug, the “rusty” sputa was speedily and permanently arrested, and the attack in half the cases aborted; in others so shortened as to recover in six or seven days. This remedy, he states, acts as promptly in pneumonia as in hæmoptysis, whether used hypodermically or “per ora,” and in a few hours arrests the “rust” by relieving the intense congestion on which it depends. He usually combines it as follows:—*R. fl. ext. ergot, f. ʒiv.; tr. digitalis, f. ʒj.; plumbi acetatis, gr. vj.; aquæ cinnamomi, ad. f. ʒij.* Sig. Give a tablespoonful every two hours until bloody sputa stops—then twice a day. He begins the treatment by an antipyretic dose of quinine—from 40 to 60 grains—which, in connexion with the ergot mixture, equalises the pulmonary circulation, relieves the congestion and inflammation, and the patients recover in about half the time required by other modes of treatment. Believing that this treatment far excels all others in its rapidly beneficial results, he presents it to the profession for trial. [Ergot, contrary to what Dr. Wells supposes, has been previously employed in the treatment of pneumonia. In 1875, the *N. Y. Medical Record*—the paper in which Dr. Wells' communication appears—contained an abstract of a paper by Dr. Wycisk from the *Allg. med. central Zeitung*, which was also published in the *Irish Hospital Gazette* of the same year (p. 175). In five cases in which Dr. Wycisk used ergot early, none ended fatally, none became chronic, and none left appreciable deposits behind them. In all of them the exudation was decidedly checked by the ergot.—ED., “PERISCOPE.”]

SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P.

VITAL STATISTICS

Of Eight Large Towns in Ireland, for Four Weeks ending Saturday, April 19, 1879.

Towns	Population in 1871	Births Registered	Deaths Registered	DEATHS FROM ZYMOTIC DISEASES							Annual Rate of Mortality per 1,000 Inhabitants
				Smallpox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhoea	
Dublin, -	314,666	787	756	37	8	24	4	7	21	1	31·3
Belfast, -	182,082	534	493	—	10	8	1	21	18	7	35·3
Cork, -	91,965	193	203	—	—	1	—	2	1	3	28·8
Limerick, -	44,209	104	112	—	—	—	—	4	3	6	33·0
Derry, -	30,884	82	48	—	2	—	—	—	—	—	20·5
Waterford, -	30,626	72	106	—	—	—	1	—	2	—	45·0
Galway, -	19,692	28	46	—	—	—	—	—	—	—	30·5
Sligo, -	17,285	39	28	—	—	—	—	2	2	4	21·0

Remarks.

There has been again a perceptible diminution in the death-rate of the large towns generally, owing to the advance of spring. The mortality, however, was still very high in Waterford, Belfast, Limerick, Dublin, and Galway. It was also high in Cork; but in Sligo and Derry it was low. The death-rate for the period was 27·3 per 1,000 of the population annually in London, 19·5 in Edinburgh, 24·4 in Glasgow, and 32·1 within the municipal boundary of Dublin. Omitting the deaths of persons admitted into public institutions from localities outside the registration district, the mortality in Dublin was at the rate of 30·2 per 1,000 annually. In this city 128 deaths were caused by zymotics, being about eight deaths below the average number due to this class of diseases in the previous ten years (136·5). The percentage of all the deaths caused by zymotics was 16·9. Smallpox is declining quickly, the deaths from the epidemic being 37, compared with 62 in the preceding four weeks. At the close of the period 101 smallpox patients remained under treatment in the Dublin hospitals, against 135, 177, 188, 122, 86, and 47 at

the close of the six preceding periods respectively. Measles and fever were somewhat less fatal, while scarlatina showed a slightly increased fatality. The 21 deaths attributed to fever were classified as follows—typhus, 7 deaths; enteric, 13 deaths; continued fever of undetermined type, 1 death. Whooping-cough continues to be very fatal in Belfast, and is seemingly widely diffused throughout the country. A gradual subsidence in the mortality from diseases of the respiratory organs is taking place in Dublin. The deaths from this class of affections numbered 184, being equal to 24·3 per cent. of the total mortality. They included 136 from bronchitis, and 33 from pneumonia, the averages in the previous ten years being—respiratory affections generally, 168·9; bronchitis, 128·8; and pneumonia, 22·8 deaths.

VITAL STATISTICS

Of Eight Large Towns in Ireland, for Four Weeks ending Saturday, May 17, 1879.

Towns	Population in 1871	Births Registered	Deaths Registered	DEATHS FROM ZYMOTIC DISEASES							Annual Rate of Mortality per 1,000 Inhabitants
				Smallpox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhoea	
Dublin, -	314,666	734	790	37	27	26	1	7	25	9	32·7
Belfast, -	182,082	618	441	—	2	3	3	28	14	15	31·5
Cork, -	91,965	182	173	—	—	—	—	2	5	6	24·5
Limerick, -	44,209	89	100	—	—	—	—	2	4	4	29·5
Derry, -	30,884	102	77	1	1	—	2	—	—	1	32·3
Waterford, -	30,626	79	45	—	—	—	—	—	—	—	19·3
Galway, -	19,692	32	38	—	—	—	—	—	4	1	25·0
Sligo, -	17,285	30	33	—	—	—	—	1	—	—	24·8

Remarks.

A very high death-rate prevailed in Dublin, Londonderry, Belfast, and Limerick; a moderately high death-rate in Galway, Sligo, and Cork. In Waterford alone the mortality was low. It was at the rate of 23·6 per 1,000 of the population annually in London, 19·4 in Edinburgh, and 22·7 in Glasgow. Omitting the deaths of persons admitted into public institutions from localities outside the district, the death-rate in the Dublin registration district was 31·5 per 1,000, while it was 32·6 per 1,000 within the municipal boundary of this city. Dublin had the highest death-rate of all the Irish towns, and the mortality from zymotic

diseases was also very high. They caused 167 deaths in the four weeks, compared with a ten-years' average of 134·4 in the corresponding period. Smallpox, measles, scarlatina, and fever were especially fatal. Of the 25 deaths returned as due to fever, 9 were ascribed to typhus, 12 to enteric, and 4 to continued fever of undetermined type. At the close of the period there were 102 patients suffering from smallpox under treatment in the Dublin hospitals, compared with 101, 135, 177, 188, 122, 86, and 47 at the close of the seven previous periods respectively. Whooping-cough still rages as a severe epidemic in Belfast, where diarrhoea was also very fatal. The deaths from affections of the respiratory organs amounted to 157 in Dublin, and included 118 from bronchitis and 26 from pneumonia. The averages of the ten preceding years were 123·6 deaths from diseases of the organs of respiration generally, 88·1 deaths from bronchitis, and 20·8 deaths from pneumonia.

METEOROLOGY.

*Abstract of Observations made at Dublin, Lat. 53° 20' N., Long. 6° 15' W.
for the Month of April, 1879.*

Mean Height of Barometer,	-	-	-	29·707 inches.
Maximal Height of Barometer (on 29th at 9 a.m.),	-	-	-	30·385 „
Minimal Height of Barometer (on 7th at 3 p.m.),	-	-	-	28·913 „
Mean Dry-bulb Temperature,	-	-	-	44·0°
Mean Wet-bulb Temperature,	-	-	-	41·6°
Mean Dew-point Temperature,	-	-	-	38·8°
Mean Elastic Force (Tension) of Aqueous Vapour,	-	-	-	·237 inch.
Mean Humidity,	-	-	-	82 per cent.
Highest Temperature in Shade (on 25th),	-	-	-	59·6°
Lowest Temperature in Shade (on 3rd),	-	-	-	31·2°
Lowest Temperature on Grass (Radiation) (on 3rd),	-	-	-	26·9°
Mean Amount of Cloud,	-	-	-	·68·7 per cent.
Rainfall (on 17 days),	-	-	-	1·997 inches.
General Direction of Wind,	-	-	-	E.N.E.

Remarks.

A generally cold, dull month—the mean temperature being about 4° below the average of the preceding 13 years. The weather of April was, in fact, quite in keeping with the unprecedented winter of 1878–79, so that a more backward spring has not been experienced for many years. The first few days and the end of the month were fine; but the period from the 5th to the 25th, inclusive, was characterised by changeable wintry weather. Deep atmospheric depressions passed over Ireland on the 6th and 7th, the barometer falling to 28·913 inches in Dublin at 3 p.m. of Monday, the 7th. At this time a steady rise of the barometer was in progress over Scandinavia, indicating the approach of easterly

or polar winds to the British coasts. At 4 p.m. of the 9th a sudden rain set in with a freshening N.E. breeze in Dublin, and next day the polar air-current became fully established. On Saturday, the 12th, the weather turned bitterly cold, and in the evening heavy showers of hail and snow fell. On Easter morning (the 13th) the ground was thickly covered with snow in Dublin. After a cold, raw forenoon, heavy snowstorms and a fresh E.N.E. gale prevailed in the afternoon. Very inclement weather continued until the 17th, at 3 20 p.m., of which day the relative humidity of the air was as low as 62 per cent. About the 21st more seasonable weather, as regards temperature, set in; but an almost constantly overcast sky, with damp and gloom, suggested memories of November rather than of April. On the 24th the westerly, or equatorial, air-current finally displaced the easterly, or polar, air-current, so that temperature rose and the sky became clearer. At 5 30 p.m. of the 25th a vivid flash of lightning and peal of distant thunder were followed by a heavy shower. The weather was dry from the 26th to the end of the month. A lunar halo was visible at 9 p.m. of the 3rd; a solar halo at 12 30 p.m. of the 14th. Rainbows were seen on the 5th, 24th and 25th. The atmosphere was more or less foggy on the 17th, 21st, 23rd and 24th. Hail fell on the 7th, 11th, 12th, 13th, 14th and 15th—snow or sleet on the four last-named days. The crescent moon and the planet Venus shone brilliantly in the western sky on the evenings of the 24th and 25th.

EMOLLIENT INJECTIONS IN GONORRHOEA.

DR. BAUER, of St. Louis, says:—"I rarely resort to internal treatment, since the disease is simply local. The injection which I apply in the acute cases is as follows:—℞. Inf. sem. lini (ex 3 iij. parati), ʒ vj.; cui adde ext. opii aquosi, fl. gtt. xvij. M.S. To be injected warm every three hours and retained for a few minutes. This injection is not only emollient, but sufficiently viscid to cover the bare urethra with a protecting coat, and sufficiently narcotic to soothe the irritated nerve papillæ. It is advisable first to clean the urethra with a warm water injection. Without exception, the patients at once felt relieved, and the discharge commenced to diminish. Toward the end, the injection might be alternated with a very weak solution of the acetate of lead, say one-third of a grain to the ounce of liquid, which seems sufficient to dry the secreting surface and to complete the cure.—*Lond. Med. Record*, May 15, 1879.

PERISCOPE.

Edited by G. F. DUFFEY, M.D., F.K.Q.C.P.

A CASE OF PROGRESSIVE MUSCULAR ATROPHY WITH SCLEROSIS OF THE LATERAL COLUMNS.

DR. J. C. SHAW records the case of a man, forty-four years of age, whose illness began with fibrillary twitchings of the small muscles of the hands, followed by wasting, which progressed rapidly, and was equal on both sides. Three months later atrophy and fibrillary twitchings began in the muscles of the arms and forearms. Bulbar symptoms appeared three months later, soon followed by weakness and atrophy of the muscles of the legs. There was no anæsthesia, no rectal or vesical difficulty. The atrophied muscles were the seat of soreness, and pain was caused by stretching or pressing on them. What muscular tissue remained reacted normally to faradisation. There was no contracture, but fibrillary twitchings were present. The patient died from exhaustion, two years from the commencement of his nervous symptoms. The spinal cord and brain only were examined. To the naked eye the brain seemed healthy, as did the cord externally, but section showed increased vascularity of the gray matter. "The anterior nerve roots were apparently not diminished in size, but presented an unusually translucent appearance." After hardening in bichromate of potass sections were prepared, by Clarke's method, from various parts of the cord. There were found—1, Atrophy of the ganglionic cells of the anterior nerves; and 2, sclerosis of the lateral columns. The atrophy of the nerve cells was almost complete in the cervical region, less marked in the dorsal, and still less so—although still manifest—in the lumbar region. The disappearance of the cells seemed to be preceded by pigmentation, and the processes were first to waste. The sclerosis was of a very light (*sic*) character. The nerve fibres in the diseased part were a little smaller than natural, had a cloudy appearance (due to an albuminous exudation), and a tendency to run together. The connective tissue septa was somewhat thickened, and there was some increase of spider cells, with undue vascularity. The vessels were healthy in structure, but their lymphatic sheaths were distended by an albuminous exudation which stained fully in logwood. The sclerosis in the cord seemed to occupy pretty accurately the lateral pyramidal tracts of Flechsig, and was symmetrical on both sides. It was extensive in the cervical region, and gradually diminished, attaining its minimum of extent in the lumbar region. No morbid condition was observed in the anterior columns. The sclerosis was traced, gradually diminishing into the medulla oblongata, where, at the level of the middle of the olivary bodies, it involved a great part of the anterior pyramids, and a considerable tract on each side, lying external and anterior to the nuclei of the

hypoglossal nerves, and two narrow bands running directly from behind forwards, connecting these tracts with the pyramids. The exact point at which the disease ceased does not seem to have been determined, but sections through the middle of the pons and crura showed nothing abnormal. There was atrophy of the cells of the nuclei in the medulla oblongata, chiefly of those of hypoglossi and vagi. The anterior nerve roots do not seem to have been examined. In the pectoral muscle diminution in size of the muscular fibres alone was noticed.

The author argues this case is not one of amyotrophic lateral sclerosis, in the sense of Charcot. He, rather arbitrarily, restricts the term sclerosis to a "peculiar active and primary" affection of the neuroglia cells. In his case, he considers that the primary affection was the atrophy of the motor nerve cells of the anterior horn, and that the affection of the white tracts was a secondary degeneration, such as occurs after brain lesion, and in which the "nerve tubes are the primary seat of trouble."—Reprint from *Journal of Nervous and Mental Diseases*, Jan., 1879. [We fail to understand completely the argument of Dr. Shaw. He appears to contend—1st, that his case is not similar to those described by Charcot, because it differs in clinical history, absence of contracture (state of tendon reflex is not mentioned), and in *post mortem* appearance—slight implication of lateral columns; and, 2nd, that Charcot has wrongly described the clinical history and pathology of amyotrophic lateral sclerosis, because this description does not agree with the case under discussion. Compare the abstract of Flechsig's recent papers, on the Pathology of Amyotrophic Lateral Sclerosis, by Dr. Ringrose Atkins, in his "Report on Nervous and Mental Diseases" in the number of this Journal for May, p. 410, *et seq.*—REP.] J. M. P.

TRACHEOTOMY IN INFANTS UNDER ONE YEAR OF AGE.

As the reported cases of successful tracheotomy performed during the first year of life are but few in number, the two following cases, reported by Dr. Elias, of Breslau, are of unusual interest. The first case was that of a delicate girl, ten months old, who was suffering from a severe form of diphtheria. The tonsils, uvula, and posterior wall of the pharynx were covered with a thick membrane. There was marked foetor ex ore, and suffocation was imminent. The improvement after the operation was slow, but on the sixteenth day the canula was permanently removed, and on the twenty-first day the tracheal opening had closed, the voice was pretty clear, and the general condition was excellent. On the following day, however, the child was suddenly seized with violent convulsions, in one of which she died on the same evening. The autopsy showed that the tracheal wound was firmly closed, and revealed no abnormality in the respiratory organs or the intestines. From this negative result Dr. Elias concludes that the fatal convulsions had no

direct connexion with the diphtheria, but that they were probably due to dentition, and favoured by the anæmia resulting from the severe sickness the child had just passed through. The second patient was eight months of age, and was suffering from membranous croup. Tracheotomy was performed on the second day of the disease, with immediate relief to the threatening symptoms. The tube was removed permanently on the thirty-third day, and a few days later the wound in the neck was entirely healed. From his experience in these two cases, Dr. Elias warmly advocates an early operation in children under one year of age, when the signs of suffocation are threatening. He recommends the employment of canulæ of, at most, from 8–8.3 cm. in length, and 4 mm. in thickness, with a curve of from 1.7–2 cm. radius. The outer canula should have an oval fenestrum. Longer canulæ irritate unnecessarily the mucous membrane of the trachea. As early as possible the patient should be made to breathe through the larynx; this can be done by removing the inner canula and stopping the external opening of the outer canula with the finger. This manœuvre should be kept up each time until dyspnoea sets in. In this way the children will soon become accustomed to breathe through the mouth, and both canulæ can then be definitely removed without danger.—*Deutsche med. Wochen.*, Nov. 9, 1878, and *N. Y. Med. Record*.

THE TREATMENT OF SPRAINS BY HOT WATER.

DR. BUNTON (*Philadelphia Medical Reporter*, Jan. 18) writes:—"There is one application which I will tell you of, and recommend to you: it is the use of hot water. I do not know that it is particularly new, but I am sure that it is not in as general use as it might be. A good many years ago I was one morning passing a theatre, where, standing on the steps, I saw that most excellent surgeon, since gone, Dr. Paul B. Goddard. He called to me, and asked me if I knew how to treat a bad sprain, adding, "Come in, and I will teach you something." I went in, and then found that a star foreign actress—a large, heavy woman—had just turned her ankle, was suffering greatly, was unable to take her part in rehearsal, and it was feared would not be able to sustain her rôle in the evening's performance. Dr. Goddard directed her to place her foot and ankle in a bucket of hot water, when more hot water was slowly poured in from a kettle, until the highest endurable temperature was reached. The limb was then retained in position for some fifteen minutes, the pain gradually decreasing until it passed away. As the pain vanished, the motion returned, and the lady was enabled to play her evening's part, without a limp and without pain. This case made a great impression on my mind, and I tell you the anecdote for the sake of the moral contained—viz., the early use of very hot water in sprains, particularly of the ankle joint.—*Lond. Med. Record*, May 15, 1879.

NEW PREPARATIONS AND SCIENTIFIC INVENTIONS.

A New Medical Microscope.

The choice of a microscope is an important matter to the medical student, because of the difficulty of getting a good one for histological purposes, and of changing an unsuitable one once it is bought. Most prefer Hartnack's model, or one of its English imitations, but so far, in the opinion of competent judges, the latter cannot be compared with the former. There is, however, a medical microscope made by Messrs. Parkes & Son, of Birmingham, which is an exception to this rule.

After a full trial of its powers, extending over a considerable time, I can say with confidence that I do not know any English or Continental microscope which at all approaches it at the same price.

1. It is equal to Hartnack's in the definition and other qualities of its objectives.

2. The stand is stronger and more durable.

3. The objectives can be changed without the trouble of screwing and unscrewing.

4. There is no dispersion of light after it reaches the hole in the diaphragm, as the latter is made to slip up flush with the surface of the stage.

5. The field is very large (without sacrificing definition), and flat to the edge.

The powers furnished with this instrument are a $1\frac{1}{2}$ and $\frac{2}{3}$ inch combination, and a $\frac{1}{4}$ th, which, with the respective eye-pieces, A and B, give a magnifying power ranging from 80 to 558 diameters.

There is also a graduated draw-tube; and a scale of magnifying powers is supplied with each instrument.

The whole is packed in a polished lock-up mahogany box, furnished with a leather handle, which makes it very portable.

The objectives, instead of having to be screwed up in brass boxes, fit into a leather-lined compartment of the box itself.

The accompanying woodcut will give an idea of the form of this instrument.

I have thought it right to bring an instrument which has given some of my medical friends and myself the greatest satisfaction, under the notice of the readers of this Journal, as the makers do not advertise, preferring to let their microscope make its way by its own merits.

CHARLES A. MACMUNN, B.A., M.D., Dub.,
Wolverhampton.

Impermeable Gloves.

These impermeable films of India-rubber may be most strongly recommended both for the purposes of general practice, of accouchement, and of the dead-house. Instances are now almost beyond counting in which medical men have performed *post mortem* examinations on subjects pregnant with infected poisons, and have themselves been infected and subsequently suffered severely, and in many cases fatally, from pyæmia. Likewise, in the course of accouchements, many a practitioner during vaginal examination has become infected and suffered permanently for the rest of his life. It would be a very wise precaution if practitioners should decide invariably to use the impermeable glove when making *post mortem* examinations, and in all doubtful cases of vaginal examinations. The makers of these gloves are Cow Hill and Co., of Cheapside. Although impermeable, as their name implies, the gloves are of a filmy thinness, so that they do not interfere with the delicacy of touch, and their use is, we believe, compatible with a very delicate perception of surface.—*Lond. Med. Record*, May 15, 1879.

Perfumed Iodoform.

A solution, which was presented at the Dublin Pharmaceutical Conference, was prepared by shaking tincture of iodine with a fragment of fused potash, until the colour was removed, and covering the odour of the iodoform produced by the addition of eau de cologne or lavender water. Lint dipped in this solution and afterwards dried, proved a pleasant and efficient application to indolent sores.—*Lond. Med. Record*, May 15, 1879.

THE DOCTOR
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